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[Continued from SUPPLEMENT No. 142, page 2265.]

THE ART OF PRESERVING THE EYE-SIGHT

Adapted from the French of ARTHUR CHEVALIER.

X.—OF THE NUMBER AND FOCI OF LENSES, AND THEIR MOUNTINGS.

SPECTACLE LENSES are classified according to numbers which indicate their foci. As the curve of a sphere is in inverse ratio to its radius, the shorter the latter the stronger the curve, and *vice versa*. Thus a No. 20 lens has less power

FIGS. 77, 78, 79.—TERMINAL SHAPES OF BOWS.

than a No. 18; the lower the number the stronger the lens, and *per contra* the higher the number the weaker the power.

Into all treatises the tables of classification of lenses as proposed by Charles Chevalier in 1841 have been copied, and these are the ones now in general use.

CHEVALIER'S GRADUATION OF SPECTACLE LENSES.

Myopy.

1st *Series*, beginning with No. 30, used generally by those who wear spectacles for the first time.—60, 30, 20, 18, 16; feeble myopy.

2d *Series*, in more general use.—15, 14, 13, 12, 11; more pronounced myopy.

3d *Series*, yet frequently used.—9, 8, 7, 6, 5, 4½, 4; in bad cases of myopy.

FIG. 76.—SPECTACLES WITH SIMPLE BOWS.

4th *Series*, rare and exceptional cases.—3¾, 3½, 3, 2¾, 2½, 2, 1¾, 1½, 1; worst cases of myopy.

Presbyopy.

1st *Series*.—60, 48, 36, 30, 24, 20; beginning stages.

2d *Series*.—18, 16, 15, 14, 13, 12; second degree.

3d *Series*.—11, 10, 9, 8, 7, 6, 5; well pronounced cases.

4th *Series*.—4½, 4, 3½, 3, 2½, 2, 1¾, 1½, 1; worst stages.

Up to the present time the graduation of spectacle lenses is still made in inches; thus, a lens No. 20, is one of 20 inches focus. Chevalier has advocated the application of the decimal system to this graduation, but it has not been brought about, since it would necessitate the renovation of such an immense amount of material. In the present state of things, however, the absence of a decimal graduation may be remedied by employing intermediate foci, the use of which is indispensable in certain cases; for between certain foci there is often too great a difference. Thus, from 30 we pass to 24, from 24 to 20, etc. Meanwhile, 27 and 22 are

FIG. 80.—× SHAPED BRIDGE.

foci which we cannot do without. The following is the table of intermediate numbers that we described in 1859.

ARTHUR CHEVALIER'S SCALE OF INTERMEDIATE LENSES FOR MYOPY AND PRESBYOPY.

Weak myopy or presbyopy.—90, 76, 66, 54, 42, 33, 27.

Middling or bad cases of the same.—22, 19, 17, 15½, 14½, 13½, 12½, 11½, 10½.

Very bad cases of the same.—9½, 8½, 7½, 6½, 5½, 4¾, 4¼, 3¾, 3¼, 2¾, 2¼, 1¾, 1¼.

For hypermyopy and for cataract, the lenses should be graduated by lines; in this way we succeed in choosing the proper focus with mathematical accuracy. Those who do not look at the question of spectacles from a mathematical standpoint, or who have not studied it sufficiently, might regard such precautions as puerile; but on this subject we have made such conclusive experiments that we do not fear to advance the opinion that such a proceeding is very necessary.

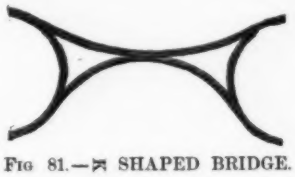


FIG 81.—X SHAPED BRIDGE.

MEANS OF DETERMINING THE NUMBERS OF SPECTACLE LENSES.

Having described the way in which lenses are made, their classification by number, and their forms, it becomes necessary now to know how to make the choice of a number for

FIG 82.—SEMICIRCULAR BRIDGE.

any particular case, in such a way as to adapt it perfectly to the visual trouble that is to be corrected. However excellent a lens may be as to quality, it may become an instrument of the most harmful nature if ill adapted to the kind of affection that it is designed to modify.

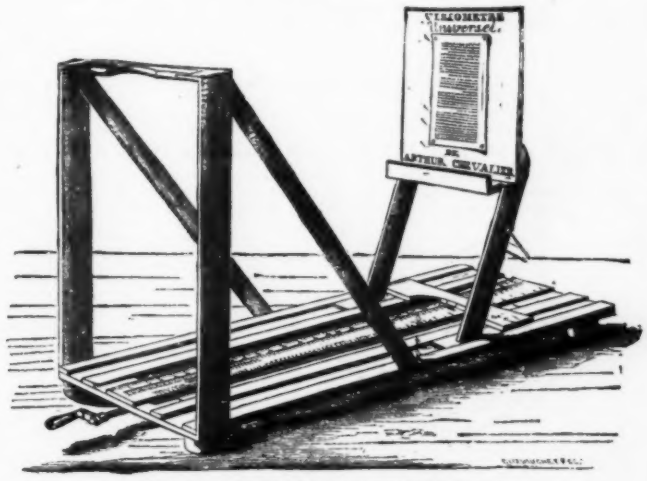


FIG. 75.—CHEVALIER'S VISIOMETER.

To determine the numbers of lenses, then, several instruments have been devised, called *Optometers* or *Visiometers*. Young, Scheiner, Juval and others have constructed excellent apparatus for this purpose.

The best procedure, in our own opinion, is to make use of the formula described in treatises on physics; and which, for presbyopy, consists *in multiplying the distance of normal vision* (12 inches) *by the distance of distinct vision, and dividing the product by the difference between the two.* For example: A long sighted person (presbyope) *ought* to read at 12 inches, but reads in fact only at 24 inches.

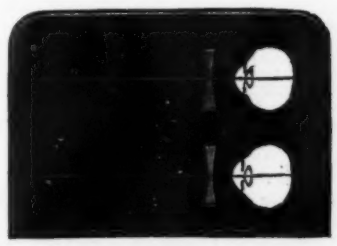


FIG. 83.—SHOWING PROPER POSITION OF THE LENSES WITH REGARD TO THE PUPILS.

$$\text{We will have } \frac{11\text{x}24}{24-11} = 20 \text{ plus a fraction.}$$

He will need, then, a lens of 20 inches focus to make up the deficiency of refraction. For a short-sighted person (myope) we make the same calculation. For instance, a person *should* read at 12 inches, but *can* read only at 6 inches. We will have, then, this formula:

$$\frac{11\text{x}6}{11-6} = 13 \text{ plus a fraction.}$$

FIG. 84.—EYE-GLASS *A POULIE.*

☞ *The numbers indicated by these calculations are those for looking at near objects. To see distant objects it will be necessary to double the number indicated for long sight, and take half of that indicated for short sight.*

In the examples given the presbyope will need No. 40 for

FIG. 85.—EYE-GLASSES.

distant objects, and the myope No. 6½. The presbyope, then, uses a number half weaker for distant vision, and the myope does just the contrary. This is very readily explained when we come to recall the theory of these two kinds of sight.

Having in every case proved the exactness of the rule before mentioned, we have made an application of it in the instrument represented in Fig. 75, which we have named the "*Universal Visiometer.*"

FIG. 86.—HAND EYE-GLASSES.

On a horizontal divided scale runs a vertical screen, which the patient moves by means of a screw, while resting his forehead against the standard in front of the instrument, stopping the screen as soon as it has reached such a distance that the printed characters may be read perfectly. It only remains to ascertain the number on the scale; which will be that of the focus

FIG. 87.—HAND EYE-GLASSES.

needed. While this apparatus may render great service, it is somewhat cumbersome, and we have therefore reduced it lately to its simplest expression, by using a flexible meter measure of ivory, on one side of which are engraved centimeters and millimeters, and on the other the numbers which correspond with the distances determined by

the foregoing formula. The *visiometer* is graduated so as to indicate a still greater number of intermediate figures, the use of which is indispensable; for the question of spectacles will not be definitely solved until the foci of the lenses will have been especially calculated for each visual affection.

For extreme presbyopy other means are adopted for finding the proper number, since it is impossible to apply the rule to this stage of long sight.

OF SPECTACLE FRAMES.

Spectacles are composed of *body* and *bows*. The body is formed by the circles which hold the lenses, and are connect-

Figs. 88, 89.—LORGNONS, OR QUIZZING GLASSES.

ed by the bridge, which rests on the nose. The bows serve to fix the spectacles to the temples by passing over the ears.

The circles are made both round and oval; the former are unsightly, and it is much better to adopt the oval, taking care to give it sufficient dimensions; for the frame should not be perceived in looking through the glass. The bows are

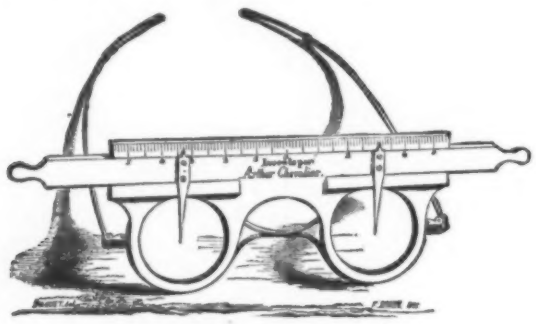


Fig. 90.—THE PUPILLOMETER.

simple (Fig. 76) or compound, and their extremities are either spatula-shaped (Fig. 77) or made in the form of a loop (Fig. 78). The latter hold better, but are apt to get entangled in the hairs and to pull them out. Sometimes the bows are bent as in Fig. 79; this shape is in common use. As no two noses have the same form, the bridge of the spectacles should vary according to the wearer; thus persons whose nose presents no sensible curve will use an ⋊ shaped bridge (Fig. 80); those whose nose exhibits a pronounced curve will use a ⋉ shaped bridge (Fig. 81), or a half circle (Fig. 82). There is nothing more important than that of choosing a setting. It

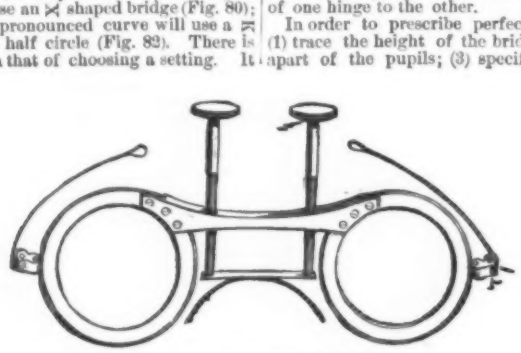


Fig. 91.—THE AXOMETER.

will be understood that the same kind of setting will not serve indifferently for everybody. It should vary with the more or less prominent projection of the bridge of the nose, the distance apart of the eyes, and the breadth of the forehead. If the nose is strongly aquiline and the bridge slopes but little, the center of the glasses will appear above the optical axis. If the glasses are not exactly the same distance apart as the eyes, the optical axis will be within, or outside of, the center of the lenses. Finally, if the breadth of the forehead stretches the bows apart, they will not only hold poorly, but the body of the instrument will bend, and the parallelism of the eyes and the glasses will no longer exist; in all such cases distinct vision will be impossible.

It is absolutely necessary that the frames and settings should be made in such a manner that the center of each

Fig. 92.—THE BESICLOMETER.

lens will correspond with the visual axis (Fig. 83), for otherwise the wearer runs the risk of injuring his sight. It is important to remark that the lenses should never be placed at a distance from the eyes; for then the focus changes, and the sight may then be enfeebled. People very often scorn such advice, however, and wear their spectacles on the extreme end of the nose.

The materials used for spectacles are steel, silver, gold, tortoise-shell and horn. Steel, because of its flexibility, is much used; gold and silver are good metals for spectacles; but those having the body of tortoise-shell and bows of silver or gold are preferable, as they are not so apt to become fatiguing. In most frames the lenses are set in grooves made in the circumference of the circles; sometimes the groove is made in the lenses, and the circle fixed into them; in this way are constructed the spectacles "*à poulie*" (Fig. 84) used mostly by myopes. *Spectacles* are best for fixing the glasses near the eyes; for if they are well made, the parallelism and centering are perfect. However, in many cases *eye-glasses* with an elastic bridge (Fig. 85) are well suited for short reading, for examining things quickly, etc. There is a kind of eye-glasses made, too, which are furnished with little moveable plates that adapt themselves to the sides of the nose, and thus cause the instrument to hold more firmly. Eye-glasses are made of the same materials as spectacles; but we prefer those made of tortoise-shell and having a gold spring, the pressure of the latter being quite light.

The hand eye glasses are represented in Figs. 86 and 87. They may be used for reading, when this is not too prolonged, and for looking at distant objects.

The *lorgnon*, single eye-glass, or, as it is sometimes called, "quizzing-glass" (Figs. 88 and 89), should never be used, as it always tends to make the eyes unequal in power.

It is often difficult to fit the frames exactly, especially on children. The best thing to do in difficult cases is to take an exact measure, and have the frames made expressly for the individual. In order to take such measurements we have devised three instruments: 1st, the "*Pupillometer*;" 2d, the "*Axometer*;" and 3d, the "*Besiclometer*."

The *pupillometer* (Fig. 90) is designed for measuring the distance between the eyes. The subject places it before his eyes, then the optician slides each index along the scale till it appears directly in front of the pupil. The distance apart is then seen in numbers on the scale.

It is often difficult to ascertain of what height the bridge of the spectacles should be, in order that the lenses may be properly centered with regard to the pupils. This is ascertained by means of the *axometer* (Fig. 91). The patient having put on the spectacles, the movable bridge of the instrument is lowered or raised until the pupils are seen in the center of the circle. This done, the axometer is placed on a sheet of paper, and the bridge, along with the lower half-circles, is traced thereon with a pencil. The breadth of the forehead is taken with the *besiclometer* (Fig. 92), which indicates the proper breadth of the spectacles from the extremity of one hinge to the other.

In order to prescribe perfect frames, then, we should (1) trace the height of the bridge; (2) indicate the distance apart of the pupils; (3) specify the breadth of the forehead. Such frames furnished with lenses of pure *crown glass* will prove the *ne plus ultra* of spectacles.

HANDLING AND USE OF THE MEAT OF DISEASED ANIMALS.

By T. S. Sozinskey, M.D., Ph.D., of Kansas City, Mo.

To any medical man who has made himself familiar with the method of business and common practices of butchers and beef and pork packers, but especially of the latter, there can be scarcely any doubt in his mind that a great deal of meat is handled by them which is unfit for use. There may not be much substantial reason for great alarm about the matter, but there is at least some cause for it. The brief remarks I am now about to make on this important subject, for the edification of the readers of the *Reporter*, are based on observations and inquiries recently made by me, principally in and about Kansas City, one of the great stock trade and packing centers.

It is impossible to gather much information from butchers and packers as to the condition of the animals slaughtered by them; of course they will not expose any of their doings which would not bear criticism. To obtain information on this point, the best place to glean it is at the stock yards where they obtain most of their supplies; and even there it is hard to get at the actual facts. Now, on visiting the stock yards during warm weather, one sees, at times, dozens of cattle and hogs which are overcome with the heat, which are sinking or have sunk from sunstroke. What becomes of these unfortunate animals? If they are able to stand on their feet at all, and sometimes even when they are not, they are sold to butchers and packers. I greatly fear, too, that sometimes animals that have actually died are dressed and sold. There is only too much reason for believing that some of the people who traffic in meat obey that part of the Mosaic code which succeeds the edict, "Ye shall not eat of anything that dieth itself," and which says, "Thou shalt give it unto the stranger that is in thy gates, that he may eat it; or thou mayest sell it unto an alien." Now I think I am putting it mildly enough when I declare that the meat of animals which are on the point of dying when they reach the butcher's hands is unwholesome. Our knowledge, however, as to the precise effects of such meat is very limited; it is far from what it should be.

Remarks somewhat similar to those just made might be made about the cattle and hogs which are prostrated while in transit, on account of being huddled together for days, in railroad cars, without food of the right kind and in sufficient amount. The yards in West Philadelphia are a very good place for observing how these "cripps," as the stock men call them, are disposed of.

I now turn to a more shocking part of my subject. When the hog cholera, Texas cattle fever, or any other serious epizootic disease breaks out in a lot of marketable animals, they are disposed of as soon as possible to butchers and packers, and generally at but a slight sacrifice. If the animal is able to hobble to the slaughtering place, it is considered fit for use. This statement is not any too positive, for I have taken pains to know whereof I speak. Now, can it be that the flesh of animals suffering from serious zymotic diseases, such as hog cholera and Texas cattle fever, is wholesome? I certainly doubt it, and I know there are very few who would eat such meat, if they knew it to be such. But it must be said that little or nothing is known as to the effects on the human system of the meat of animals butchered while suffering from blood diseases; it is only surmised that they are bad. Surely this is a matter which merits thorough scientific investigation. I think the Government should take it in hand.

I may say a few words about packing measled pork. In the packing establishments no inspection is made for the detection of this condition, which is by no means sure. I have been told that it is always detected incidentally, which may or may not be so. Thus, it seems that when a ham is put in warm water, which is done in the process of curing, if measled it will be evident from the red spots which appear. Even when the measled condition of the meat is apparent, I doubt much whether it always finds its way into the rendering tanks.

From what precedes, it is evident that a weighty responsibility rests with beef and pork packers and butchers; in a measure, the health, the life, of almost every one is in their hands.—*Med. and Surg. Reporter.*

THE OLDEST MAN IN THE WORLD.

The stories of men whose age is considerably over a hundred generally come from districts in which verification is impossible. Never, perhaps, even from such districts, has a candidate for the age of one hundred and eighty years been presented, but one is offered for our credence in the person of a citizen of Bogota, in San Salvador. We are told that he only confesses to this age; but his neighbors, who must be better able to judge, affirm that he is considerably older than he says. He is a half-breed, named Michael Solis, and his existence is testified to by Dr. Hernandez, who was assured that when one of the "oldest inhabitants" was a child this man was recognized as a centenarian. His signature, in 1712, is said to have been discovered among those of persons who assisted in the construction of a certain convent. Dr. Hernandez found this wonderful individual working in his garden. His skin was like parchment, his hair as white as snow and covering his head like a turban. He attributed his long life to his careful habits; eating only once a day, for half an hour, because he believed that more food than could be eaten in half an hour could not be digested in twenty-four hours. He had been accustomed to fast on the first and fifteenth of every month, drinking on those days as much water as possible. He chose the most nourishing food, and took all things cold.—*Lancet.*

PROPOSED TREATMENT FOR ANÆMIC GIRLS.

M. L'Abbe Moigno, the able editor of *Les Mondes*, prints the following communication from a lady of Geneva, and, believing it to be a sensible idea, hopes that some one may be induced to try the suggestions she puts forth:

Every one knows, she says, that butchers, however lean and delicate they may have been before learning their trade, rapidly gain a fresh and ruddy complexion, corpulency of body, and a general state of well being. To what should such a marvelous result be attributed? It may perhaps be answered that it results from the "choice cuts" that they carefully put by for their own personal use. This, however, is mere speculation. But it is certain that the majority of persons who engage in the butchering business lose their appetite. Would not this state of general good health be rather attributed to assimilation (by means of the respiratory functions) of the nutritive juices of meats which become volatilized in contact with the air? Would not this be considered a species of *nourishment* by *affusion* of particles of blood and meat diffused through the surrounding air? Everything seems to lead to such a belief. And if this be the case, would it not be possible to derive from the vicinage of well aired, properly organized meat markets, or abattoirs, a complete system of hygienic treatment for chlorotic, anæmic or chloro-anæmic girls, and especially for children of a feeble or lymphatic constitution? It seems to me that this is an idea that ought to engage the attention of physicians.

To Make Watch Hands Red.—Mix to a paste over a lamp 1 oz. carmine, 1 oz. chloride of silver, and ½ oz. tinner's japan. Put some of the paste on the hands, and lay them face upward on a sheet of copper, holding it over a spirit lamp until the desired color appears on them.

Polishing Powder for Gold Articles.—A polishing powder, highly esteemed by the gold-workers of Germany, consists of sesquioxide of iron (iron rust) 70 per cent., and sal ammoniac 20 per cent. To prepare it, protochloride of iron, obtained by dissolving iron in hydrochloric acid, is treated with liquid ammonia until a precipitate is no longer formed. The precipitate is collected in a filter, and, without washing, is dried at such a temperature that the adhering sal ammoniac shall not be volatilized. The protoxide of iron precipitate at first becomes charged with sesquioxide.

HISTOLOGY AND THE CELLULAR THEORY.

By DR. EDOUARD FOURNIÉ.

Translated for the SCIENTIFIC AMERICAN SUPPLEMENT, from the *Gazette des Hopitaux.*

IV.

Second Point.—We have just proven that, in order to substantiate the formula *omnis cellula a cellula*, it is not allowable to liken the ovule to a cell. Let us now see whether it is true that every tissue arises from the *proliferation* of blastodermic cells. Some days after fecundation, the embryo is entirely formed of cells. The cells, it is true, present morphological characters, differing according to the blastodermic layer in which they are found, but they are none the less cells, and we do not meet, in the midst of them, with any other substance, formed or amorphous. This fact is incontestable, and apparently the argument is more favorable to the theory *omnis cellula a cellula;* but we must interpret it judiciously. The complete living being, being the result of a series of transformations of organized matter, this last has been subjected to certain determined forms, favorable to successive transformations, and it is that which is most diffused during the embryonic period. But is that form the only one? Has it, in any other way, the fundamental signification that M. Virchow accords to it? Such are the questions that must be resolved.

No, the cellular form is not the only one of which the *formative function* avails itself in order to attain its ends. The various organic systems are hardly only designed in the embryo, as we see many tissues develop themselves which have no genealogical relation with the cellular elements. It is thus that the sheath that surrounds the notochord (chorda dorsalis) forms itself from a state of membrane, without our being able to discover in its formation any cellular element. It is the same with myolemma, with perinervine, with the capsule of the crystalline lens, the membrane of Descemet, the glandular walls proper, and in general with every fundamental membrane designated by Todd and Bowman under the name of *basement membrane.*

It is thus, moreover, that amorphous substances formed by living organized matter, and entering as an essential element into most of the organs, have not any cellular production. The hyaline substance which separates, in the embryo, the *interapophysiary masses*, and which later will become the cartilage of the transverse and intricate apophyses, does not inclose any cellular element. The *amorphous cerebral* substance which appears at the same time as the myelocytes, and before the formation of the bodies of the nervous cells and cylinder axes, is by no means of cellular production, and above all should not be confounded with the laminary tissue, as M. Virchow has confused it, in designating that substance under the name of *neuroglia.*

The reasons that M. Robin gives in order to show the error of M. Virchow are altogether demonstrative. The *myeline*, the fundamental substance of mucous tissue, are amorphous substances which have no other relation with cells than the relations of neighborhood. The *plasma* of the blood, surely an amorphous substance, which shows itself in the cardiac tubes as soon as that organ commences to contract, has not any genealogical relation with the *hæmatic globules* or any other cellular body.

It is thus, finally, that the calcareous and chitinous apparatus, which play so great a role in the anatomical constitution of certain animals, represent non-cellular masses, not having any relation of origin with the proliferating segmentation of the cells.

The facts which precede are authentic; they have been pointed out by men whose competence is not to be doubted.* We are then authorized in concluding that the cellular form is not the only one (contradictory to what is advanced by M. Virchow), of which the *formative function* avails itself in constructing the living being.

But this question of geometric form would be indifferent to us—for it must necessarily be that organic matter must assume some form or other—had not M. Virchow accorded to it the very primest importance in considering the cell as the fundamental organic unit, and the point of departure for every physiological and pathological phenomenon. It is from this new point of view that we shall now investigate the question.

M. Virchow is convinced that every cell is necessarily constructed of an *enveloping wall* and of a nucleus. This manner of looking upon it is very seductive, and it may easily be conceived that M. Virchow has allowed himself to be carried away in considering the cell, thus *imperfectly viewed*, as the basis of all vital phenomena. If, indeed, all cells were constituted of an enveloping membrane and of a nucleus, we should be obliged to recognize the cell form as a distinct element, altogether characteristic, and whose physiological destiny is beyond parallel. Unfortunately for M. Virchow, this is not the case, and it is to-day more than proven that most cells have no envelope. The blastodermic cells, in particular, are unprovided with an envelope. The consequences of this fact are most unfavorable, they could not be more so, to the cellular theory. From the moment, indeed, that the cells have no envelope, they are no longer cells, but simple globular corpuscles, constructed of a nucleus which is infolded by some kind of protoplasm, and the *cell*, in so far as it is a distinct organ, no longer exists but as an exceptional state. From the moment that the envelope does not exist, the cell is not "the only elementary form which serves as the basis of every vital phenomenon," for, before the cell, there are the formed and living corpuscles, which by a sort of special molecular attraction are grouped around the nucleus; there is also the nucleus.

When, for example, the corpuscles of the vitellus dispose themselves in such a manner as to form the vitelline nucleus, and the spheres of segmentation, it is not by virtue of the cellular form of the ovule that these phenomena are produced; we can attribute them to the natural activity of the corpuscles, and when the vitelline material is entirely segmented in such a manner as to form a mass of corpuscles in the cellular form, shall it be said that this new form is henceforth the base of vital phenomena? No, certainly not; for this new form is itself governed by the special activity of the elements that gave birth to it. Elsewhere, this form is eminently transitory, and should only be considered as a momentary *anatomical convenience.*

The cells of epithelium, which are the true cells constituting the permanent tissues, are developed in the fœtal period in accordance with a process analogous to that of the segmentation of the vitellus. In the same proportion as the cells of the superficial beds fall in desquamation, they are replaced by others which develop spontaneously in the deeper beds in the midst of the amorphous substance. At first they are simply nuclei infolded in non-divided protoplasm, not segmented into cells; then appear the plans of division which limit the bodies of the cells.* Here again, and though later on the cellular body incloses itself in a true envelope, the said form of the cell is altogether accessory, for it is preceded and is entirely governed by the special activity of the nucleus, and by that of the protoplasm.

The nerve cells of the nervous centers only borrow from the embryonic cells their nuclei; the latter become segmented in the midst of the amorphous cerebral substance, and they are the new nuclei that constitute the myelocytes, from the state of free nuclei, and the cellular myelocytes when they have added to themselves an enveloping membrane. As regards the ganglionic cells and the great sympathetic, they begin evidently by the formation of small nuclei, around which is developed, later on, a cellular body.† Here, again, the so-called cell form can only be consided as accessory.

Finally when the members commence to be developed in the embryo, cells of vitelline production no longer exist, and how, then, can we explain the complete formation of these organs? Observation shows that the embryo-plastic nuclei serve as the center of generation for cells of cartilage, of fibers, etc., etc.‡

The facts that we have just enumerated prove clearly that the so-called cell form does not have the importance that M. Virchow attaches to it; they prove that the organic activity shows itself in the elements even before their grouping, under the globular form; they show finally that, whether it be regarded in the embryo, or whether it be considered in the permanent tissues, the so-called cell form borrows its special activity from the elements that are grouped under the globular form, and only represent, definitely, an *anatomical and physiological convenience.* In the permanent tissues it is favorable to the construction and renewal of membranes; in the embryo, it realizes better than every other way the division of physiological labor.

After having proven that the organic cells should not be considered as *vital units* assimilable to the animal; after having demonstrated that the cell is not the only form of vital activity; after having demonstrated that the *cells do not bear, each in itself, the complete characteristic of life;* after having shown, finally, that cells cannot produce directly another cell, we may limit our criticism, for it is more than is necessary to demonstrate the slight value of the hypotheses on which the cellular theory rests. Meanwhile, as this theory has been the point of departure for other hypotheses which reigned among us in physiological and pathological investigations, we will continue our task to the end.

We will be excused if we neglect to speak of the famous *cellular territories* conquered so easily by M. Virchow on the domain of J. Goodsir. The English have done ample justice to that plagiarism.

"It is in activity that we find the characteristic of life. . . . According to what we know, this action, this vital activity, is not raised up in any part of the body by an innate cause (*causa innata*), immanent and wholly contained in any one part. Everywhere, to obtain vital activity, there must necessarily be an excitation. Every vital activity supposes an excitation, or, if you prefer, an *irritation.* . . .

"Every time that a special activity is awakened, it is to cause it to functionate, to nourish or to form a part: function, nutrition, formation. . . .

"In a large number of tissues the true function escapes us; we know very little of the special function of the tissues of the conjunctival substance and of that of the greater part of the epithelial elements. We can say well enough what is their use in such or such a case, but they appear to us most often to be inert bodies, availing little or nothing to the true function, forming above all the support for the diverse parts of the body, covering the surfaces, uniting, gathering, or separating, according to circumstances.§

"It is not nutrition that is permanent, it is the possibility of nutrition *as long as life persists.* By the action of irritants nutrition, which was *possible*, becomes *real;* in other words, irritants provoke the manifestation of the property. . . . I have designated by the usual word, irritability, the property of living bodies that renders them susceptible of passing into a state of activity under the influence of external agents.‖ Every part undergoes certain states of fatigue, which prevent the part from producing the quantity of movement that it had produced up to that time. But to be able to perform a function anew, these fatigued parts have not the necessity of fresh nutrition; to give back to them the powers that they have lost by fatigue, it suffices to leave them in a state of repose for a certain time." ¶

In reading these propositions, in which M. Virchow has given a *résumé* of the whole of this physiological doctrine, we cannot help but notice the insufficiency of the notions of general physiology that the author possesses. He has that in common with others, with all those who, governed, absorbed in the establishment of innumerable facts of histology, have disdained the study of scientific tradition, finding it simpler to improvise at once, and in presence of the microscopic elements, an easy theory, destined, in their eyes, to replace general physiology. On such a fragile basis the theories do not hold long together. Such is highly true as regards the theory of M. Virchow, as we shall now demonstrate in examining successively each one of his propositions.

1st. It is not proper to say that *activity* is the characteristic of life, because all matter, placed under certain conditions, is active.

We well know that philosophers would seek to quarrel with us over the interpretation of the word *activity*, which, by them, was always applied to the operations of the soul and never to inanimate matter; but we are altogether obliged to be in our own time and to speak the language spoken by *savants.* Now, to-day, it is the custom to speak of the *activity of matter*, because all activity being only appreciable by the movement that it provokes, we have come to confuse the *cause* with the *sensible effect*, the movement with the cause that makes it possible. We feel ourselves at ease on this ground, and, to prove that it is not unknown to us, we ask permission to quote from the text the prolegomenas of our *Physiologie du Système nerveux.* It will not be useless elsewhere.

"Movement is the expressive manifestation of everything that *is.* That this motion may be molecular or organic, that it may be shown by the laws of physics, of chemistry, or of physiology, it exists fastened to matter of which it is one of the indispensable conditions. We cannot conceive of matter stable in its mass or in its elements; this untold spectacle would be the annulment of actual life or a creation resting on new conditions.

"Light, sound, heat, electricity, are modes of motion; the earth, so imposing in its majestic proportions, moves in space, not only in its totality, but in each one of its parts, and the intimate molecular movement of which it is the seat is not the less powerful; it is to it as the hand that prepares the marble for our statues, the stone for our houses, the combustible for our manufactures and our hearth; again, it is indebted to it, as we are indebted, in going through successive transformations, the indispensable aliment of plants, of animals and of man; finally everything moves, and there is nothing, even to that inert body, that we call a cadaver, which may not be the seat of a molecular movement characterizing a new aggregation.

"Motion is then one of the indispensable conditions of everything that occupies a place in space. But this movement, whence comes it? What is it? Is it a peculiar force inherent in the material element and resulting from its special constitution? or even the material element, is it only the *substratum* of a force already existing, which impresses on it movement and life? No, motion is not the principle that animates matter; motion is only the mode expressive of its activity; it is by it that we become cognizant of this very activity itself, not in its principle, in its essence, but in its effects. A principle, a force, isolated from matter, is inaccessible to our senses, and escapes *directly* from all our means of investigation; our mind can only fix itself on the effects that result from the action of force on matter, and, as motion, and a peculiar motion, is the necessary consequence of that action, it follows that motion, by its infinite varieties, furnishes us the notion of matter being all that it is, *and this ought to be the realizable end of all scientific research.*"*

(*To be continued.*)

* Ch. Robin, *Anatomie et Physiologie cellulaires*, passim. H. Bennett, *Leçons cliniques sur les Principes et la Pratique de la Médecine;* French translation by Lebrun; Paris, 1873.

* Ch. Robin, *Anatomie et Physiologie cellulaires*, p. 303.
† Robin, *loc. cit.*, p. 412.
‡ Robin, *loc. cit.*, p. 352.
§ *Loc. cit.*, p. 329 et seq.
‖ Virchow, quotation of M. Robin in *Anatomie et Physiologie cellulaires*, p. 344.
¶ *Loc. cit.*, p. 344.

* Edouard Fournié, *Physiologie du Système nerveux cérébro-spinal*, Prolegomenas, p. 1. Paris, 1872.

EFFECTS OF MEDICINE IN SMALL DOSES.

Read before the Section on the Practice of Medicine, American Medical Association.

By JOHN MORRIS, M. D., of Baltimore, Md.

WE have been for years gradually lessening the doses of medicine that we administer to our patients. A lesson has been furnished us by homeopathy, however irrational it may be, as far as the matter of giving less medicine is concerned, and many of us who have studied the effects of remedies in different doses have discovered that very small quantities will produce very striking results. The experiment of giving small doses can be safely tried in nearly all the cases of disease which we are called on to treat. Practically, as some writer suggests, these diseases may be divided into three forms:

1. Those that recover by natural means, that is, under the influence of time, rest and abstinence.

2. Those that no treatment can benefit, it matters not how scientific or rational it may be.

3. Those that can be benefited by judicious treatment, and in the cure of which the best possible judgment must be exercised, not only to save life, but to avert evil results.

It is only in diseases of the last named class that remedies can be usefully employed, and the deductions embraced in this paper are based on the results obtained from their administration in these cases.

Before proceeding to discuss individual remedies, we beg to state that we entirely agree with Sir Edward Blaine, in the opinion that remedies are relative agents, and that their virtues cannot be fairly essayed or beneficially ascertained by trying their effects on sound subjects, and that morbid conditions are actually necessary to test their true value. And, further, we desire to be understood that all the inferences contained in this paper are the result of our own clinical experience.

We will now proceed to examine into the effects of

CALOMEL.

When we first commenced the practice of medicine, more than thirty years ago, it was the custom of practitioners generally to prescribe calomel in large quantities; indeed, in the Western and Southwestern States of the Union, it was frequently administered in teaspoonful doses. Calomel given in this way does not, as a rule, cause salivation; for the reason that very little of it is absorbed. As in the cases of large doses of iron and bismuth, as we shall hereafter show, the greater part of it is carried off by the bowels. Calomel will, however, by its very weight, act mechanically, and in this way will frequently arrest obstinate bilious vomiting, when all other remedies have failed. It is a very remarkable circumstance, too, that it will, in very minute doses, control the persistent vomiting of pregnancy; indeed, I know no remedy so efficient in this condition as one-twelfth of a grain of this chemical given three times daily, for a period of a week or ten days. No enlightened medical man, at this day, prescribes calomel simply as a purgative. If administered at all, it is given to procure its well known alterative effects, or its action on the liver through the influence it exercises on the duodenum. In our own practice we have found no agent so prompt and so specific in its action on the mucous membrane as calomel in small doses. This is peculiarly evident in diseases of the alimentary tract in children. The administration of mercury is never carried, in our day, we believe, to the point of actual salivation. All that is deemed necessary is to have the system thoroughly influenced by the remedy, as evidenced by the breath and tenderness of the gums, and this is best accomplished by one grain doses given three times daily, in combination with opium. As an alterative, very small quantities are undoubtedly the most efficacious. Plummer's pills, than which no better remedy can be given in tertiary syphilis and chronic rheumatism of a syphilitic character, contain but a half-grain of calomel each. The pill of the British Pharmacopœia is still weaker. The bichloride of mercury too, in small quantities, exercises a remarkable influence over affections of the mucous surfaces. Ringland has called attention to its potency in dysentery.

No one who has had any experience can be in doubt as to the power of calomel in the treatment of disease. To secure, however, its just and true influence, it must be administered with great care, and in small doses. Unfortunately it is too frequently given empirically, and without any accurate knowledge as to its effects. It is truly the great searcher of the human system. There is no secretion that it does not stimulate and promote; the mucous surfaces, the liver, the kidneys, the skin, the glandular apparatus, and every part of the body, are reached by it. That it has fallen into disrepute as a remedy, in the last quarter of a century, there is no doubt. This, we believe, is owing entirely to its use in large and unnecessary quantities. In safe hands and

in small doses it still is, as it will ever be, a most valuable agent. Headland says that very minute doses can be given in debility, and even in scrofula, in which cases it acts as a tonic by stimulating the functions of the liver. He also recommends the administration of very small potions of the bichlorate of mercury in chronic syphilitic affections, if we wish to secure the special blood action deemed necessary to destroy the poison in this disease. We entirely concur with him in this opinion.

IRON.

Iron is, perhaps, the most popular remedy of the day. It is administered in nearly every form of disease, to all classes of people, and for every variety of trouble in which anæmia is supposed to hold a place. This indiscriminate prescription of iron has grown up in the last few years, and surely needs some correction. The doses given are entirely too large, and unnecessarily tax the digestive functions. This is apparent from the fullness about the head frequently caused by its use.

But a very small portion of iron can be absorbed, and this only by the aid of the acid of the stomach; and if large doses be given, the greater part, if it act at all, must act mechanically, after the manner of bismuth; but the presumption is that it does not act at all, and is carried off as so much waste material, through the intestines. This is shown by the character of the stools. Iron is not truly a tonic; it is merely a restorative. Even in some cases of anæmia it is not a restorative, save in the very smallest quantities. This is owing to the system being so debilitated that it is incapable of assimilating the remedy, and carrying it to its true destination. The tincture of the sesquichloride of iron is decidedly the best preparation we have in liquid form, when given in proper doses. The muriatic acid adds greatly to its efficacy, by its tonic and diuretic effects, apart from the power this acid has of rendering the metal more soluble. Sulphate of iron in large doses is an irritant, but in small quantities proves one of the most certain and powerful of the preparations of this metal. In administering iron it should always be borne in mind that it is not a catalytic agent, and can be only useful to the extent of its absorption into the blood and its action on that fluid. Dyalized iron, which is a hydrate of iron containing no acid, will, no doubt, in the future, be the only form employed, on account of its being readily absorbed, and its freedom from the bad effects of all preparations of this mineral. Although a strong solution, it possesses no unpleasant taste; does not produce constipation, nor disturb the digestive powers. Its discovery we esteem a great boon indeed.

QUININE.

Quinine is, perhaps, the most potent and unerring of all our remedies, though its precise mode of action does not appear to be thoroughly understood. Like calomel, this chemical has been and is still administered in immense doses in the Southwestern and Western States of this country. This we believe to be very bad and dangerous practice. In ordinary cases of remittent and intermittent fever, not of a congestive character, doses of two or three grains, repeated every two hours, are sufficient. A toxic dose of twenty or thirty grains may forestall an attack of chill and fever, but the reaction that consequently follows leaves the nervous system in a state of depression, and produces a train of very unpleasant symptoms. We are convinced that the continued use of quinine in small doses, for a number of days, or even weeks, or months, if deemed necessary, will be more likely to effect a permanent cure in cases of malarial fever than a large quantity given in one or two potions. In severe neuralgias large doses of quinine are well tolerated, especially if given before the expected time of attack. In these cases the administration of twenty or thirty grains, by causing cinchonism, appears to more promptly and effectually break up the chain of nervous trouble than the same quantity given in divided doses. In the very aggravated forms of pyrexia, too, quinine may be given in large quantities without the usual bad effects following its administration. The great advantage of small doses of quinine is that they can be given to patients in high fever, without causing irritability of the stomach or adding to the general excitement of the system.

ALCOHOL.

Alcohol, too, like quinine, is given in too large doses. A small potion administered every hour more efficiently balances the nervous system, and brings about defervescence by lowering the temperature, than a large amount given at a single dose. The great evil of giving alcohol freely is, that after its influence passes off there is a state of prostration left which compels its continuance or the substitution of some other drug. Alcohol given in small doses acts as a temporary stimulant without producing any secondary sedative influence. In large doses its action is entirely narcotic; a fact that is apparently overlooked by many practitioners, who appear to be wholly ignorant of its physiological effects. As a catalytic agent in cases of poison from the bite of reptiles, very large quantities may be given with impunity, and also in fevers dependent upon the presence of a blood poison in the system. In all other instances small doses, given at proper intervals and with due regard to the temperament of the patient, are not only the safest but the most effectual.

DIGITALIS.

The action of digitalis is not always appreciated by those prescribing it. In large doses it is an exceedingly unsafe remedy, save in delirium tremens and other allied disorders. It irritates the stomach and produces vomiting. This action is independent of its depressing power on the heart, which is produced by the influence it exercises over the functions of the vagus nerve. In small doses, say fifteen or twenty drops of the tincture, digitalis is one of the best cardiac tonics we possess; and in the same quantity it acts most efficiently in those cases of dropsy which result from obstruction of the cardiac circulation. Its depressing power should be always borne in mind and guarded against. In Great Britain half-ounce doses of the tincture are administered, with advantage, in cases of mania-a-potu, but an abnormal condition of the nervous system exists in this disease, which requires enormous quantities of all remedies to produce sedation. The true office of digitalis is that of a cardiac tonic, and its prescription in future will be, no doubt, confined to cases of heart trouble dependent upon obstruction of the circulatory system. In view of this fact its well known efficacy in combination with calomel and squills can be readily understood. Its depressing and irritant effects in large doses interfere with, if not totally destroy, its power as a remedial agent.

TARTARIZED ANTIMONY.

Tartarized antimony is a valuable agent which has fallen greatly into disuse of late years. Administered in small doses its action is most marked and beneficent. Apart from its catalytic effect in the blood, it acts as a sedative, diaphoretic and expectorant. To produce its true power, it must, according to Laennec, be absorbed in the blood, and this can only be brought about by the administration of minimum doses. Its efficacy in small quantities, as an adjunct to purgative remedies, through its power of relaxing the muscular fiber of the intestines, is very generally known. At one time tartarized antimony entered into the composition of every febrifuge and expectorant mixture, but owing to its being prescribed in too large potions, and consequently producing great vital depression, it lost character as a medicinal agent. This loss of character was, perhaps, also partly due to the great revulsion that has taken place in the practice of medicine in the last quarter of a century. The abandonment of tartarized antimony we conceive to be a very great error. We prescribe it daily in very small doses, with the happiest results and believe, when properly given, it possesses special merits. In disregarding its use the profession, in our judgment, has lost one of its most useful remedies.

IPECAC.

The effects of ipecac on the economy are very similar to those of antimony, save that it does not act on the blood, and therefore, cannot be ranked as an antiphlogistic. As a neurcotic, diaphoretic and expectorant, its power is unmistakable. Its influence on the mucous membrane, in minute doses, is most interesting. We have seen cases of persistent vomiting, which had continued for days, arrested by small quantities of this drug, all other means having failed. Very large doses of ipecac have been suggested recently in dysentery, and its use in this way, if we are to believe the journals, has been very successful. We have had no personal experience in this matter, but we can readily understand in what manner nauseating or emetic doses of ipecacuanha might, by their relaxing effect on the whole system, influence the rectum itself. Small quantities of ipecac, in combination with opium, have almost a specific action in dysentery, particularly if they follow the administration of a saline aperient. In no form is the potency of small doses of ipecac so apparent as in the troche or lozenge, where its specific action on the mucous surface of the throat and bronchi becomes at once evident.

ALOES.

Aloes is another drug not prescribed with sufficient caution. Very small quantities of Barbadoes aloes, particularly when administered with an alkali or some vegetable extract, will act as a gentle aperient. Larger doses, as given in the olden time, are drastic in their action, and leave very disagreeable after results. A striking peculiarity in the action of this cathartic, not generally known, is that an increase of the quantity given beyond a medium dose is not attended by a corresponding increase of effect. Three grains of Barbadoes aloes, made into a pill with half a grain of nux vomica and a grain or two of Castile soap, will, if taken after dinner or before going to bed, produce a gentle daily movement of the bowels without griping or other unpleasant consequences. In these cases the action of aloes is due to the power it possesses of stimulating the stomach. Good rhubarb, too, prescribed in the same manner and quantity, will produce a like result. These are our usual prescriptions for persons of sedentary habits suffering from constipation. Rhubarb is not as good an agent as aloes, for the reason that it possesses astringent qualities, and its use is often followed by a torpid condition of the bowels. Rhubarb is tonic, astringent or aperient, according to the manner and quantity in which it is prescribed.

OPIUM.

Opium is the next remedy the action of which is to be considered. The indications for its administration are as follows: pain, muscular spasms, insomnia and nervous excitation. In small doses opium acts as a stimulant and sedative: in large doses as a narcotic. Small doses frequently given, in case of insomnia, are much better, owing to their accumulative power, than larger ones which stimulate and produce a condition of restlessness and excitement. Save for the relief of actual pain, opium should be prescribed very cautiously, for the reason that it exercises so great an influence by arresting the secretions, and thus interfering with all the processes of life. It is only necessary to administer an eighth or a quarter of a grain to produce its peculiar action on the skin and mucous membrane. This quantity can be taken without any apparent disturbance of the economy. Opium, like quinine and alcohol, may be given in exceptional cases in large potions. Its unpleasant effects, we may here remark, when thus prescribed, such as constipation, nausea, headache, etc., may be greatly modified by its combination with belladonna or hydrate of choral. Opium, in very minute doses, combined with ipecac, proves one of our best expectorant remedies. Administered either internally or in the form of troches, it stimulates instead of arresting the mucous secretions of the air passages.

Unfortunately the true power of opium is not generally understood, and it is therefore frequently administered unwisely. Its normal influence is exercised over the nervous system. It produces no change in the blood. Its effects occur immediately on absorption, and in this we believe it stands alone among our remedies. To secure this peculiar effect it must be given in minute potions, as narcotism, in the slightest degree, interferes with all its beneficent influence.

GELSEMIUM.

Gelsemium is a much abused agent, owing to a want of knowledge of its therapeutic properties. In England, however, we are glad to know a thoroughly scientific investigation has been entered into, and we may in the future expect a wiser prescription of it, based on an accurate acquaintance with its qualities as a remedy. In this country it is given by many practitioners in unnecessarily large doses, and frequently, instead of producing the desired effect on the capillaries, causes a toxic condition, very agreeable, and sometimes alarming. As an antiperiodic, it acts in a manner similar to quinine. The after effects of both, if large doses be given, are poisonous, cinchonism being produced in one case and narcotism in the other. Gelsemium, however, has to be much further tried before it can be classed as a special agent.

SQUILL.

Squill, in small doses, acts as a sedative, though it is never, we believe, given to exercise this special influence. Its power as a diuretic and expectorant is generally recognized, but it is not prescribed in this country to the same extent that it is in Great Britain. Given in doses exceeding one or two grains, it is irritant in its effects, and sometimes proves emetic.

ERGOT.

Ergot is a remedy that is prescribed very indiscriminately. Large doses should only be given in cases of hemorrhage of the uterus, where its specific action on that organ is speedily desired. Its influence over the capillarics is best secured by small quantities given at intervals. This remedy, like many others, is accumulative in its action.

Arsenic, aconite, belladonna and iodide of potassium are also remedies the action of which we would be glad to discuss, but the limits we have marked out for this paper preclude our doing so at this time. Without attempting, however, an analysis of their particular qualities, we venture the opinion that their true physiological effect can only be obtained by the administration of very moderate or even minute doses.

Before concluding, it may not be out of place to state that the action of many remedies is greatly modified or increased by combination with other agents. When we find combinations in nature producing remedial or curative effects, the elements entering into them are always small in quantity. What more agreeable or effectual remedies have we than the natural waters found in the springs in different parts of the world? And yet how small a quantity of the various salts is discovered in these waters by analysis. The same may be said of sea air, than which no more potent influence can be named. Would it not be well for us to take a lesson from nature in this as in many other matters? It would be well to bear in mind that there are a number of substances the efficacy of which is increased by admixture, and one or two the action of which is qualified by combination. The aperient effect of certain drugs is augmented by the addition of henbane, belladonna and nux vomica. The nausea and depression following the use of morphia, as we before mentioned, are almost entirely obviated by combining it with hydrate of chloral. Opium in very small quantities prevents the griping of many cathartic remedies, without lessening their potency. In giving large doses of calomel, salivation is prevented if this mineral be mixed with bicarbonate of soda or chlorate of potash. This last combination does not appear to be a chemical one, or *scientific*, at first glance, but I am assured by capable chemists that no bad effects result from its use, and that a chloride of mercury is not really formed. Iodide of potassium supplements treatment by mercury, and in those cases where there exists a lingering of the poison it appears most beneficial. The emetic effects of tartrate of antimony are concentrated by its admixture with opium. It was formerly largely given in this way in cases of delirium tremens, and is still used in Ireland in protracted labor due to rigidity of the os uteri. Trituration, too, may be mentioned as a means to increase the power of medicine. This is a means very little understood. There are many other combinations modifying the action of medicinal agents, but they are no doubt familiar to you all. Our only purpose at this time is to call the notice of the profession to the efficacy of small doses of certain unmistakable remedies in daily use, and to invoke a greater watchfulness in the administration of drugs, as well as closer attention to their effects when given in large or small quantities.

DEDUCTIONS.

1. That the true physiological effect of remedies may best be obtained by the administration of small doses frequently repeated.
2. That medicines thus given are accumulative in their operation.
3. That the effect of remedies is greatly increased by combination, the manner of preparation, the time and mode of administration, etc.
4. That large doses of medicine frequently act as irritants; that they produce an abnormal state of the blood, as is evidenced by such conditions as narcotism, alcoholism, iodism, ergotism, bromidism, etc.
5. That more special attention should be given at the bedside to the influence of remedial agents, to the end that a greater certainty may be exercised in their prescription.

TREATMENT OF DISORDERS ARISING FROM PROLONGED MENTAL DEPRESSION, ETC.

By J. H. O'Reilly, M.D., of the United States Marine Hospital Service, Louisville.

That a prolonged continuance of depressing mental emotions is productive of numerous disorders, both real and imaginary, is a fact in medicine, thoroughly established. Conspicuously common among the affections arising from this source, which may be arranged under the former title, are sleeplessness, vertigo, neuralgia, and general nervous prostration, palpitation of the heart, and other disturbances of the circulatory apparatus, gastralgia, the different varieties of dyspepsia, and obstinate and annoying constipation. Under the head of imaginary may be collected those numerous and vaguely defined complaints everywhere recognized and denominated as hypochondriacal.

The people of the United States are just passing through a condition of financial depression which is unparalleled for duration and for the universality of distress thereby occasioned. As a consequence of this state of affairs, the diseases of which mention has just been made are observed to occur with remarkably increased frequency. Taking advantage of the seemingly golden opportunity thus offered to augment their sales, and evidently under the delusion that they were conferring an inestimable boon upon suffering humanity, the manufacturers of medicines have flooded the country with "hundreds upon hundreds" of alleged "unfailing" and truly "agreeable preparations." Notwithstanding, however, this very commendable enterprise on the part of the manufacturers, it is the generally expressed opinion of physicians that, from the almost interminable list of "new remedies" recently presented to the profession, it would be impossible to select a dozen medicines whose actions, after being thoroughly tested, have not failed to meet the expectations of those by whom they were administered.

Practitioners, in endeavoring to account for the occurrence of this anomaly, have advanced a multitude of theories, many altogether unreasonable, and not a few perfectly ridiculous. One gentleman ascribes the trouble to the "fluid extracts," many of which are rendered entirely inefficacious by the excessive heat employed during the process of their manufacture. He says, "I maintain that where much heat is used (referring to fluid extracts), especially with products of a volatile character, it is possible to dissipate the valuable part of the preparation and give an inert and worthless drug." Another physician attributes the irregularity to the improper preparation and gross adulteration of the new remedies in general. It will be remarked, however, on the one hand, that the "fluid extract" is not the only preparation by which we are so constantly disappointed, and on the other it is yet to be established that the new medicines are

not equally as scientifically and carefully manufactured, and quite as little adulterated, as any that were employed previously to their introduction. While each of these gentlemen, therefore, has directed attention to a possible element in the production of this most unsatisfactory condition of things, both have failed, either from contracted observation or fallacious reasoning, to discover the great and most obvious cause of the difficulty.

As already inferred, the troubles whose increased frequency of occurrence has been the chief instrument in bringing about the introduction of these warranted panaceas, originate from prolonged worry, overstraining and depression of the mental faculties, and it is in this class of disorders, if anywhere, that the effects are to be gotten rid of by the removal of the cause. Now, these remedies, from their very nature, can be applied only to the palpable results, instead of to the original source of disturbance, and it is on this account that benefit so rarely succeeds their administration. It must be remembered, in the management of these affections, that relief comes only subsequently to the advent of complete mental relaxation and diversion. The measures to be adopted for the induction of this latter condition are both psychological and hygienic in their character. Sleep, and plenty of it, must be procured under any circumstances. Bathing is scarcely less beneficial than rest, when regularly and judiciously employed. As a means of relief, in many instances, traveling, by reason of the constant vicissitudes and excitement with which it is attended, is to be highly commended. The same may be remarked in connection with field and aquatic sports, and out-door exercises of every description. Where the occupation of the patient renders an absence from the city difficult or impossible, nothing will prove more highly efficacious than the healthy and pleasant recreation provided for by a well-regulated gymnasium. The mental amusement and distraction attendant upon gardening, pisciculture, and the raising of pets, is especially useful where the subject is a female, or a male of delicate constitution. Minute specifications concerning the nature and employment of these measures are altogether without value, on account of the numberless differences in the histories of the cases which present themselves for treatment. To the intelligent discrimination of the physician in attendance must be left the selection of such as are appropriate to the peculiar disposition and temperament of the individual under his supervision.—*Med. and Surg. Reporter.*

THE METRIC SYSTEM.

A VERSION FOR THE KINDERGARTEN.

By A METRICAL MANIAC.

I.

HURRAH for the Meter, the jolly new Meter;
Not the long—nor the short—nor the common old teter.
But partic'lar for you and for me;
'Tis the ten-millionth part from the pole to th' equator,
With it you can measure a township or 'tater,
A kingdom, a brig, or a flea.

II.

D'ye see this 'ere cube of the tenth of a meter?
That's a liter, to gauge how much drink, by St. Peter,
You can stow in your vast lower hold;
But the Gram is the boy to hit up the doctors,
And bother the fogies, and gargle concocters,
And make them to blunder and scold.

III.

O! Let them go hang with their ounces and scruples,
Tell the graybeards to "cave" and come down to be pupils,
Like Ned, Henry, Tom and your Jack;
There's nothing so easy as to learn this nice table;
With Deka and Hecto and Kilo you're able
To bounce it all off in a crack.

IV.

The Greek words increase, like Greek brats on the Shannon,
But the Latin decrease, as shot from a cannon
Looks smaller the further it goes:
Haste! Dekagram, Hectogram, Kilogram, heavy
With Decigram, Centigram, Milligram's levy,
And GILD the bald scalps of your foes.

V.

Play you're making out bills when you're writing prescriptions,
And your cramp hieroglyphics of th' ancient Egyptians
Are improved into dollars and cents;
Now what is a gram, or what are the values
Of the devilish and blackguardly weights that we all use
Annoying the Metrical gents?

VI.

A grain's one six-hundredth of our new Gram, boys;
A Gram and a third will make, sure I am, boys,
Your old Scruple, so base and so mean;
Four Grams make a drachm, or at least very nearly,
Thirty-two will fill up the ounce quite as clearly;
Was anything 'cuter e'er seen?

VII.

Then weigh eighty Grams of the best old Jamaica
With a hundred and twenty of water and shake a-
Bout one Gram of nutmeg in clean;
Squeeze in fifteen grams next of lemon-juice pure,
Shake with ice, and you compound a mixture that's sure
A babe from it's mother to wean.

VIII.

And we'll drink to the system, the new Metric System.
Who won't take it kindly, be sure we'll assist him
By sarcasm, by nudge and by gibe.
If some unlucky fogy with this new appliance
Shall a babe or two poison, you know—it is science.
Continue to weigh and prescribe.

IX.

Drink again to our system—our superior system;
For each who will use it, we'll rally and twist him
A garland of camomile flowers;
And when every nation has learned this notation,
Crime shall vanish, and all through creation
A first-class millennium be ours.

—*Hospital Gazette.*

HARD RUBBER APPLIANCE FOR CONGENITAL CLEFT PALATE.

By THOMAS BRIAN GUNNING, New York.

WE give selections from Dr. Gunning's article in the New York *Medical Journal* of this month, which will place his improved treatment of "Cleft Palate" clearly before our readers:

"Alexander Petronius, in his work entitled 'De Margo Gallico,' and Ambrose Paré, in his book on surgery, prove

Fig. 1.

that efforts to relieve those suffering from defective palate, by applying obturators, were made over three centuries ago, and the records of the last fifty years alone show that the endeavors to supplement the congenital cleft palate have resulted in the invention of mechanical appliances which in number and variety are very remarkable; yet the 'Report of the International Exhibition of 1876,' in referring to the one now submitted, says: 'This contrivance is a very marked improvement over all previous appliances to this distressing malformation.' Now, that this simple remedy was not devised earlier is owing to mistaken views as to the movement of the muscles of the pharynx and palate, both in perfect and malformed conditions, and this notwithstanding the investigation and study of these parts by the most distinguished physiologists and surgeons."

Dr. Gunning's remarks in correction of mistaken opinions respecting the action of the muscles in deglutition, speech, etc., close as follows:

"The form of the hard palate is such that the tongue can fit it around the inside of the teeth, as in the consonant *t*. The back of the tongue also fits against the soft palate and uvula exactly, and this closure can be maintained while the upper part of the soft palate shuts off the posterior nares. This is easily tested by pronouncing the consonant *k*, in which both the nose and mouth are shut off from the larynx, until the tongue leaves the palate to allow the vowel sound to come out, when only the passage to the nose is kept shut. This double closure is made even in *kee*, in which sound the contact for *k* is on the hard palate, instead of being back on the soft palate as in *koo*. The point of the tongue goes up in *t*, the back of the tongue in *k*, and the lower lip also goes up to form *p*, the upper lip and the hard palate being passive, and the soft palate nearly so, outside of its great function in respect to voice, which is to shut off the nose cavity in all sounds of speech and song except those containing *m* or *n*. At rest, the velum leaves the passage from the nose to the larynx open."

Fig. 2.

After showing that observation of the malformed palate confirms his views, he says:

"In 1864 I had become convinced that the *superior constrictor* was incapable of any action which could prevent the use of a rigid appliance to supplement the cleft soft palate, and to the present time in no case has the hard rubber palate failed to keep its place, to give entire satisfaction, and to improve the speech in a remarkable degree."

He sums up, before giving the illustrations, as follows:

"Therefore, in brief, in view of the foregoing propositions: There being no forward action whatever of the superior constrictor muscles, a rigid plate can be worn without intermission, not only in comfort, but with improved condition of the mucous membrane, which is covered in, and of the general health, the nose being as free for breathing as in a normal condition of the parts; while the plate also enables the wearer to utilize the muscles of the cleft velum. The plate is easily made, and being of hard rubber does not deteriorate in the mouth. It is not supported by any part of the cleft, and may thus be worn from early childhood without injury to the parts, in fact its support may even lessen the cleft.

"The plate, which is held up by the teeth against the hard roof of the mouth, extends up into the cleft and thence to the back of the pharynx near the tubercle of the atlas, the end being rounded to allow the sides of the pharynx to close in during the act of swallowing. This extension into the cleft being spread out over the soft parts on each side, the ununited muscles draw up against it and close off the nasal cavity. The vowel sounds are therefore preserved from the resonance of the nose by the natural action of the muscles, while the nasal sounds are used when necessary, and the tongue is able to form all the lingual consonants, the stiffness of the hard rubber affording the best possible substitute for the muscular firmness of the natural soft palate. To apply this palate a simple impression of the hard palate and teeth, as is usually taken for the setting of artificial teeth, is quite sufficient, the extension into the soft palate being made by fitting the gutta percha pattern to the parts, without subjecting the patient to the annoyance of obtaining a plaster impression of these sensitive and mobile organs. This palate is consequently so simple that any accomplished dentist can apply it, and the patient is therefore comparatively independent.

"Early use of this artificial palate prevents unnatural action of the tongue, such as attempts to close the cleft with the tongue when the latter should be free to act in articulation, whether in speaking or singing.

"Fig. 1 gives the upper side view of an appliance for a case in which the cleft passes through the whole length of the soft palate, but does not reach the front teeth.

"Fig. 2 gives the lower front view of the plate shown in Fig. 1; when worn, the narrow part is covered on each side by the cleft soft palate, as in Fig. 4.

"Fig. 3 was taken from the cast of a large cleft through both the hard and soft palate, in a patient twenty years old. The cleft in her lip had been closed in infancy; and attempts were made to close the soft palate after the cast was taken, but the parts did not unite. The case is peculiar in the absence of the bicuspid teeth and the *central* incisor, there being only an irregularly formed tooth on the mesial side of the canine instead of two incisors.

Fig. 3.

"Fig. 4 shows the hard rubber appliance as adjusted to remedy the deformity exhibited in Fig. 3, after the wisdom-teeth and the right central had been lost through decay and the malformed tooth removed.

"The cut was made from an impression of the plate *in situ* after it had been worn more than four years, day and night."

We understand that this was three years ago; the palate is still in use, and so indispensable to the patient that she will not say a word without it.

Figs. 1 and 2 represent a palate which has been worn steadily since November, 1869. For six months previous the patient persevered in attempts to use a palate made in three pieces, two of which were of soft rubber. It, however, proved useless, in fact unbearable. The flexibility of the soft rubber unfits it for use, for as the sides of the cleft soft palate draw up in the vowel-sounds they find nothing firm to close against, and frequently get above the yielding edges of the rubber; again, the tongue cannot form the guttural consonants *k* and *c*, as the soft rubber goes up out of reach; while in eating, the food often gets above the velum, and makes it more difficult to swallow than without any appliance. But all this is remedied by the *hard* rubber palate.

Dr. Gunning's appliance to supplement the congenital cleft palate, and also his methods of treating the *acquired* injury, "fracture of the jaw," were considered so important

Fig. 4.

that they were recorded at length in the Surgical Report of the United States Centennial Commission, with remarks upon his successful treatment of the Hon. Wm. H. Seward. Many of our readers will remember that Dr. Gunning was called to Washington, at the time of President Lincoln's death, to attend the Secretary of State, whose condition was also critical, through the attempt to assassinate him.

INDIA RUBBER.

ON THE PARA AND CEARA RUBBERS AND BALSAM OF COPAIBA TREES.

A REPORT on the subject of India-rubber yielding plants in their own native homes, and comprising also an account of the collecting of balsam of copaiba, has recently been issued from the India Office. It has been furnished to the Secretary of State for India by Mr. Robert Cross, who is well known for his labors in the cinchona forests of South America, and the subsequent introduction of the plants into India, since when Mr. Cross has made more than one trip to South America for the purpose of securing seeds or plants, or both, of the best rubber producing trees, and the results of his more recent expedition for this purpose is embodied in the report before alluded to.

THE RUBBER CITY.

Mr. Cross gives a brief account of the habits of the Para people, which place he reached on the 15th July, 1876. The population, it seems, is about 40,000, most of whom are engaged in some way with the dispatch of import and export produce.

Notwithstanding the reputed fertility of the Amazon valley, nearly all the necessaries of life are imported, butter and fish from Norway, rice and flour from the United States, while sugar, coffee, and mandioca come from the southern parts of Brazil. The great bulk of the citizens are described as going about more ostentatiously dressed than the people of London; the essential costume being a fine black coat and hat, snow-white vest and trowsers, and fancy French boots. Water is supplied to the city by being carted through it in barrels, and sold at the rate of three halfpence for about 21 English pints. Dysentery, yellow fever, and various other forms of fever are said to be prevalent, and altogether Para is considered to be more unhealthy than any city in India.

The province of Para, and the islands that are scattered over the lower portion of the Amazon, are described as the great field for caoutchouc collecting. A good deal of the rubber from the Rio Negro, Madeira, and other tributaries seems to come in the form of negro-head or sernamby, while that from the Para region is the finer kind of smoked biscuit rubber, to the preparation of which greater care is given. It is indeed reported that the Para tree is a different variety, its milk leaves no very prominent stain on the hands or clothing, while the milk of some of the variety of rubber of the Upper Amazon gives a black ink-like mark to the hands and clothes of the collectors.

When once in the country, how Mr. Cross started on his mission will be best described in his own words. He says: "In order to form and establish a collection of plants, and for the purpose of making the various observations on the soil, climate, and mode of collecting and preparing the rubber, it was necessary to obtain a place to live in while so employed. Every one told me I would experience great difficulty in finding a dwelling, and this proved true. After traveling round Para and searching for about eight days I succeeded in hiring a house, but at a very high rate, as the place was large, and adapted for a family with attendants and slaves. However, it was secure, and offered every facility for my various requirements, which was important."

WHERE THE RUBBER TREES GROW.

"My next work was to examine the district where the rubber trees grow, . . . and on the 25th of July I made a preliminary journey to the region where the trees were wrought." Leaving Para, the high ground was traversed for several miles until the primitive forest was reached, a path used by caoutchouc collectors through the wood. The traveler soon came upon a large tree in a state of decay, which had been tapped many times.

From the ground up to a height of 10 or 12 feet the trunk was one swollen mass of warty protuberances and knots, covered with thick scales and hard, dry bark. This singular form of growth—the result of the practiced system of tapping—has never previously been recorded. A few minutes of careful examination soon showed the real cause of those deformities.

The collector uses a small ax-like instrument an inch broad; at each stroke he cuts through the bark and into the wood for fully an inch. Hundreds of these are made in the trunk of each tree in the course of a few years, and cannot heal under any circumstances; but a layer of wood is formed over the injured part, at the expense of the bark and general vitality of the tree. The newly-formed wood is again cut into and splintered, and so the process is repeated on each successive layer, until the trunk becomes merely a mass of twisted wrinkled wood, with very thin insipid bark. In this condition hardly any milk flows from the cuts, and, although for years a few green leaves may continue to sprout from the points of the twigs, yet the tree may be considered as dead, and, in fact, finally withers away. It is, therefore, the injury done to the wood, and not overtapping, which lessens the flow of milk, and ultimately causes the death of the tree. The cuts in the wood are, of course, unnecessary, since the milk is met with only in the bark. The healing-over process which afterward takes place is similar to that seen where a branch has been lopped from a trunk. The wood is compact and rather hard, and for this reason the tree lives on for a number of years, although cut and hacked every season; but the flow of milk becomes so lessened that many are practically abandoned for years before they die. This and several large adjoining trees were growing in moist, deep, heavy soil, of a fertile character, but quite out of the reach of any inundation.

On the margins of both the larger and smaller streams a considerable number of rubber trees were found, mixed with cacao and forest trees. Three were observed, the bases of the trunks of which were flooded to a height of one foot, yet the roots seemed to run up to the brow of the bank, and no matted roots were observed, as is the case with the willow tree when growing on the margin of a rivulet. Most of the others occupied dry situations. A number of good plants were met with beneath the oldest trees. At places where the ground was covered by more than two inches of water at flood tide seedlings did not usually grow. By far the greatest number, however, were met with in situations above the reach of the highest tides. A few of the largest trees were measured, all of which had been tapped for periods varying from five to fifteen years. Most of the trees occurring within the limits of the worked districts are tapped if possessing a diameter of six or eight inches. Regularly tapped trees, as a rule, do not exceed 60 feet in height.

PROPAGATION OF RUBBER TREES.

Mr. Cross relates some rough and ready experiments made by him with a view of proving the conditions under which the Para rubber plants may be best propagated. These experiments were conducted while the plants which he had previously collected, 2,000 in all, were being established in three cases. He says they were commenced "in order to ascertain how the tree might be readily multiplied in a rough way by any person not specially acquainted with the principles of propagation. Two separate beds, the one of brown sand, the other of decayed leaves, were formed. The terminal portion of shoots, but with a bud at the lower end, were planted in the beds in a reclining position with only two inches of the points above ground. Owing to the great distance between the buds, consequent on vigorous growth, many of the cuttings were a foot or more in length. At the same time a number were set deeply in an open vessel containing only rain water. The cuttings in the sand beds were the first to grow, and soon made strong shoots and fibers. Those in the leaf mould pushed more slowly, but developed green leaflets of great substance. The cuttings placed in the water had a small portion of tap root at the base, as the object was to determine if the roots actually develop in water alone. Within fourteen days these plants had several roots formed, and one or two rather weak growths came up, but a few days after I had thrown into the water some burnt earth and wood ashes the increase in vigor was very apparent. After these experiments I felt convinced that the Para rubber tree delights in abundance of moisture and rich fertile deposits."

HOW THE RUBBER MILK IS OBTAINED FROM THE TREES.

Mr. Cross gives a very detailed description of the tapping of the trees, the collection of the milk, and the method of preparing the rubber, all facts of great interest, and the more so because information of this kind is not really procurable in a collected form, being for the most part distributed in various books of travel. On this account Mr. Cross' report should be widely circulated. The fact remains, however, that these reports are not sufficiently known among commercial and scientific men, and, consequently, the wider diffusion of the heads of Mr. Cross' experience is, perhaps, justified in the columns of this *Journal*.

The caoutchouc collectors commence work as soon as daybreak, or as soon as they can see to move about among the trees. They say that the milk flows more freely, or in larger quantities, in the early morning, but little importance is attached to this statement; the most probable reason for the early tapping is that, as rain often falls about two or three o'clock in the afternoon, it is necessary that the work should be done early, as, in the event of a shower, the milk would be spattered about and lost.

The collector first of all, at the beginning of the dry season, goes round and lays down at the base of each tree a number of small cups of burnt clay. At the lesser trees only three or four are put, but at the larger ones from eight to twelve. The footpaths leading from tree to tree are likewise cleared of sapling growths. On proceeding to his work, the collector takes with him a small ax, for tapping, and a wicker basket, containing a good-sized ball of well-wrought clay. He usually has, likewise, a bag for the waste choppings of rubber and for what may adhere to the bottoms of the cups. These promiscuous gatherings are termed "sernamby," and form the "negro head" of the English market. The cups are sometimes round, but are more often flat, or slightly concave on one side, so that, with a small portion of clay, they may be easily stuck against the trunk of the tree. The contents of fifteen make one English imperial pint.

When the collector arrives at a tree he strikes with his ax in an upward direction as high as he can reach, making a steep, upward, sloping cut across the trunk, which penetrates the bark and an inch or more into the wood, and is often fully an inch in breadth. Frequently a small portion of bark breaks off from the upper side, and occasionally a thin splinter of wood is raised. A cup is next quickly fixed with clay against the trunk, just beneath the cut. The milk, which is of dazzling whiteness, now begins to exude. At a distance of four inches or five inches, but at the same height, another cup is similarly fixed, and then another, until a row of cups encircle the trunk at a height of about six feet from the ground. Tree after tree is treated in this manner until the day's tapping is finished. The earlier the gashing of the trees and the fixing of the cups are done the better, for the milk often continues to exude slowly for three or more hours.

The collectors vary very much in the tact and ability shown in the performance of their duties. Some take care to get good clay previously, and to incorporate it well, so that a very small portion is needed to fix the cups to the trunks; they also work with neatness and intelligence, and invariably collect large quantities of milk. On the other hand, there are some who exercise no forethought in the preparation of the clay, merely scraping up a handful when they need it. This class of collectors often have many fragments of clay and other impurities in their rubber.

THE YIELD OF MILK FROM EACH TREE.

The quantity of milk that flows from each cut varies. If the tree is large and has not previously been twice tapped, the cups will for the most part be more than half full, and occasionally a few may be quite full; but if the tree is much gnarled from tapping, whether it grows in the rich sludge, by streams, or on dry soil, many of the cups will be found to contain only about a tablespoonful of milk, and sometimes scarcely that. On each succeeding day a similar operation is carried on, with the exception that the cups are placed from six to eight inches lower down until the ground is reached. The collector then begins as high as he can reach, and descends as before, taking care, however, to make his cuts in distinct places from those previously made. When the produce of milk diminishes in long wrought trees two or three cups are put on various parts of the trunk where the bark is thickest. Although many trees are large, the quantity of milk obtained is surprisingly small. This has been described as the result of overtapping, but Mr. Cross thinks it is not possible to overtap a rubber tree, if in the operation the wood is not left bare or injured, but the collector's ax always enters the wood, and the energies of the trees are required to form new layers to cover these numerous wounds. The best milk-yielding tree examined had the marks of twelve rows of cups, all the work of one season. The rows were only six inches apart, and in each row there were six cups, so that the total number of cuts made within the period of three months numbered seventy-two. Though this tree grew in a favorable situation, and was in every respect a healthy tree, it is considered that with about two years of such treatment it would in all probability become permanently injured. There seems to be no appreciable difference in the quality of the rubber, whether collected in the dry or in the rainy season. It may be, however, that in the wet season a larger proportion of water is contained in the caoutchouc, while on the other hand a larger quantity of milk flows. The dry season is, in fact, the most suitable for caoutchouc collecting.

Two other methods of tapping, which are chiefly confined to the Upper Amazon and its tributaries, are described, the principles of which are similar to those already explained.

COLLECTION OF THE MILK.

With regard to the collection of the milk from the cups when full, it is done by a man running from tree to tree with a large calabash, into which the contents of the cups are emptied. As he pours the milk out of each cup he draws his thumb or forefinger over the bottom to clean out some which would otherwise adhere; a small quantity, in fact, does remain, which is afterward pulled off and classed as sernamby. The cups, after being emptied, are laid in little heaps at the base of each tree to be ready for use next morning. The time lost in traversing the intricate muddy footpaths beneath the trees is a serious obstacle to expeditious collecting. More than twice the quantity of caoutchouc might be collected in a fourth of the time, and at far less cost and labor, were plantations properly formed.

CONVERTING THE MILK INTO RUBBER.

To prepare the rubber, the milk is put into a large flat earthen vessel; beside this are placed narrow-necked jars about eighteen inches high and about twelve inches across the broader part; the bottoms are knocked out of these jars, they are raised from the ground on three small stones, fires are lighted in these bottomless jars, and the slight distance they are raised from the ground causes sufficient draught to promote their burning; the fires are fed by dropping pieces of wood and a handful of palm nuts alternately into the mouths of the jars, the aim being to cause a dense smoke to arise from the mouth of the jar. The mould on which the rubber is prepared resembles the paddle of a canoe; in fact, at many places on the Amazon, this is the article most frequently used; if there is much milk, and when the rubber is prepared in bulky masses, a little soft clay is rubbed over the mould to prevent the rubber adhering, and it is afterward well warmed in the smoke. The operator holds the mould with one hand, while with the other he takes a small cup and pours two or three cups of milk over it. He turns it on edge for a few moments above the dish until the drops fall, then quickly places the flat side two inches above the jar's mouth, and moves it swiftly round so that the current of smoke may be equally distributed. The opposite side of the mould is treated in the same way. The coating of milk upon being held over the smoke immediately assumes a yellowish tinge, and although it appears to be firm, on being touched is found to be soft and juicy, like newly-curdled cheese, and throwing off water profusely. When layer after layer has been repeated, and the mass is of sufficient thickness, it is laid down on a board to solidify, and in the morning is cut open along the edge on one side and the mould taken out. Biscuit rubber, when fresh, is often four or five inches thick. After being hung up to dry for a few days it is ready for market. The fact of burning palm seeds (which are said to be those of *Euterpe edulis* and a species of *Attalea*) has given rise to an opinion always stated by travelers, that the smoke produced by these burning nuts exercises some peculiar effect upon the milk by which it coagulates almost instantly.

After a careful examination of this matter, Mr. Cross expresses it as his conviction that the rapid coagulation of the milk is simply produced by the high temperature of the smoke, and that with a strong current of heated air or a good pressure of steam from a pipe a similar result would be obtained. He says, "I have no hesitation in giving my opinion that equally as good rubber could be prepared by putting the milk into shallow vessels and evaporating the watery particles by the heat of boiling water."

PROPOSED CULTIVATION OF RUBBER TREES IN INDIA.

With regard to the introduction and propagation of the Para rubber plant into India, the hottest parts and the low-lying, moist tracts, or land subject to inundations, are recommended; deep, humid land suitable for cane and coffee planting is quite suited for the tree. The Malay peninsula, Burmah, Ceylon, and Southern India are said to possess many suitable localities, the green terminal shoots of succulent growth, with the leaves fully matured, making the best cuttings. They should be cut off low enough to take in a joint at the base. When planting in dry firm land, a spadeful of soil should be turned over at each place and the cutting planted in a sloping position. It should be covered with mould to within three inches of the point. That portion above ground should rest on the earth on one side to its termination, so as not to suffer through hot sunshine. Seeds may also be planted, and the soil much improved by the addition of a handful of wood ashes with each seed at the time of sowing. In watery places, or in deep mud deposits, seeds are not recommended, as many would mould and rot. Mr. Cross gives a very detailed description of the best modes of propagation, soil, etc., and also his experience in search of the Ceara rubber, about which a good deal of interest has been manifested, the plant producing it not having been accurately known, but suspected by many writers to be identical with the Para tree. Mr. Cross succeeded in bringing home seeds and plants, and the true Ceara rubber is now proved to be the produce of *Manihot glaziecii*, which, though quite distinct from that of Para, nevertheless belongs to the same natural family—the Euphorbiaceæ. The rubber is collected in a different manner to that of Para. The outer surface of the bark of the trunk is pared off to a height of four or five feet; the milk then exudes and trickles down in an irregular manner, falling, for the most part, on to large leaves that are laid about the base of the trunk to receive it; some, however, drops on to the ground, and so often gathers up with it dust and loose stones. After several days the juice becomes dry and solid, and is then pulled off and rolled up into balls or put into bags in loose masses. The trees are badly used, the tapping being made too deeply into the wood, so that many trees are in a state of decay. From the fact that Ceara rubber occupies a good place in the market, being exported at the rate of about 1,000 tons per annum, it is to be hoped that more care may be taken of the trees, and that it may be successfully established in India. Mr. Cross suggests that in the districts of Madras, Cochin, Calicut, Cannanore, Mangalore, and Bombay, many localities possessing all the conditions essential for the growth of Ceara rubber may be found, and the plant might also be tried in the deep tropical valleys of Assam; indeed, in all the parched regions of India within the limits of coffee culture.

THE BALSAM OF COPAIBA TREE.

On the subject of balsam of copaiba, which is yielded by different species of *Copaifera*, Mr. Cross describes them as having a wide distribution, being abundant in the forests of the Amazon valley, of Guiana and Venezuela, and in the wooded littoral districts of New Granada, especially in the

States of Santa Martha, Carthagena, and Panama. The finest sort known in commerce, and called by the collectors white copaiba, is met with in the province of Para, and is shipped from Para and Maranham. Very large quantities are annually sent to the French market. Formerly the trees might be seen growing within easy access; but, owing to the method of collection practiced, it is now comparatively rare. At present a collector must make a journey of several weeks in a canoe up some of the Amazon tributaries, or penetrate into the dense forest lying between the rivers, to find any considerable quantity of copaiba. The life of a balsam collector is said to be one of the most wretched description, as he is exposed daily to the drenching rains in the depths of the forest, with often an insufficiency of food, constantly bitten by large, formidable ants, and tormented unceasingly by day and night by swarms of mosquitoes. As if to make amends for all these miseries, a collector can earn, when the trees are abundant, as much as £5 per day.

HOW THE BALSAM IS COLLECTED.

The trees grow to a great height, running up to 50 feet or 60 feet before branching, but no seedlings or young plants are to be found in the forests, for so soon as the seeds fall they are greedily devoured by a small animal about the size of a rat. To collect the balsam a hole or chamber, about a foot square, is cut in the trunk, at about 2 feet from the ground. The wood is white to a depth of 4 inches or 5 inches, after which it is of a purplish red color; indeed, the woods of all the species of copaifera have this peculiar tint. When the center of the tree is reached by the ax the balsam flows out in a current full of hundreds of little white pearly bubbles. "At times the flow stopped for several minutes, when a singular gurgling noise was heard, after which followed a rush of balsam. When coming most abundantly, a pint jug would have been filled in the space of one minute." From the fact that every chip cut out was studded with drops of balsam, it seems conclusive that every particle of the wood is highly charged with it, though the bark appears to possess none. A large tree, in good condition, is estimated to yield about 84 English imperial pints. Mr. Cross refutes the stories told by travelers that the balsam is collected by gashing the bark and plugging the space with cotton to absorb the juice which exudes, or that of closing cavities, made in the trunk, with clay or wax, to allow the balsam to accumulate, and then opening them and extracting the collected balsam. Both systems, he considers, would be practically useless. Little or no care seems to be taken to preserve balsam pure. Old jars and barrels that have previously contained grease or liquors of all descriptions, and old paraffine cans, are very much sought after, and are used without being properly cleaned.

This valuable tree, like those yielding India-rubber, has been proposed for introduction into India. The temperature required for it is similar to that required for the Para rubber tree. Wet or moist land should be avoided, and the plants should be put in the best dry loam, such as is suitable for cane or coffee planting; seedlings may be planted tolerably thick so as to shoot up rapidly, when they could be thinned out to proper distances. Mr. Cross concludes his remarks on this interesting tree in the following words: "I would not recommend the planting of these trees on a large scale with a view to early profit, as the growth would be slower than Panama or Para rubber trees. The return would, I think, be realized in about the same time as is the case with oak plantations. However, a few hundred of copaiba trees growing on the planter's estate ought to enhance the value of it. Apart from the medicinal value of copaiba, it might be well to ascertain if it would not be equal to castor oil for lubricating-machinery."—*Journal of the Society of Arts.*

RAPID SILVER PRINTING.

THE *Photographisches Wochenblatt*, by request of its correspondents, repeats the details of the so-called *Licht-paus* process, which, by reason of the rapidity with which it furnishes copies, is likely to become a favorite method with army photographers in the field, where it is of importance to print quickly in all sorts of weather. Our readers are already conversant with this plan of printing by development, which is employed in this country in enlarging; but the process here given of Fritz Haugk varies a little from that already published. We put it before our readers in the form it is given in the *Wochenblatt*.

The paper most suitable is that to be purchased from dealers under the name of "positive paper" in its normal condition—that is, without any photographic preparation upon it. Its preliminary treatment is effected with a solution made up of—

Iodide of potassium	5 grammes.
Chloride of potassium	20 "
Distilled water	500 "

The solution is filtered and poured into a porcelain vessel. To it is then added ten grammes of the finest arrowroot flour; the whole is then warmed, the liquid being stirred the while with a glass rod until it begins to boil. The mixture is then permitted to cool, and the skin formed upon the top is removed.

The sheets of paper are now treated with this solution. The best way is to fasten the paper upon a flat board, cementing the four corners with sealing wax. For spreading on the solution a broad camel hair brush is the most suitable, this being moved up and down and all over the surface of the sheet. To make the coating uniform, the sheet is rubbed over with a second brush, and finally the paper is hung up to dry.

In a closed portfolio paper prepared in this way may be kept for some days in a serviceable condition. If during this period it assumes a slightly reddish tone or becomes covered with marble markings, this is no reason why the paper should be thrown away, all those imperfections disappearing when the paper comes to be sensitized.

The sensitizing bath is composed of—

Distilled water	500·0 grammes.
Nitrate of silver	40·0 "
Citric acid	2·5 "

After the paper has been treated upon this bath it becomes exceedingly sensitive. It must not be brought into daylight even for an instant. The placing in the pressure frame under the negative, as also the examination of the print, must invariably be undertaken in the dark room.

The action of the light is not at once apparent, although it is seen by a marked change of color; the full effect of the printing only comes out on development with gallic acid. For this reason the exposure to light, or operation of printing, is very short, the time required being only so long as is necessary to obtain an impression of the outlines of the image. When this has been secured the printing frame is carried back into the dark room and developed in the undermentioned solution:

Cold saturated solution of gallic acid	1 part.
Distilled water	4 parts.

The image floated upon this solution gradually becomes vigorous and assumes a dark brown tone. It is lifted from time to time by its corners in order to look at it by transmitted light, and as soon as it appears sufficiently vigorous it is dipped into cold water, so that the gallic acid may not sink into the paper. Then, if necessary, it is put into a toning solution, and finally fixed. All these operations must be conducted in the dark room.

PHOTOGRAPHIC PRINTINGS.

By JOHN L. GIHON.

Printing, comprising items of information in relation to every class of work. Plain and Albumen, Porcelain and Carbon Work, are all treated of in this series.

WHILE heading this article I have promised too much; I cannot possibly do full justice to every specified item within the limited space allotted to me each month. I shall only treat of that which is most important, and endeavor to be entirely practical. The theory of silver printing can be very concisely stated as follows:

Silver Printing.—When nitrate of silver is brought in contact with an organic substance, the resulting compound is found to be affected by light in a somewhat peculiar way; the compound slowly darkens to a reddish tint; the exact chemical reaction that takes place is very complex to trace, but it may be accepted that an oxide of the organic matter and silver is formed. This oxide is stable, unlike the suboxide of silver, and is not acted on by fixing agents to any great extent.

Albumen in Printing.—The most important of the organic substances used in printing is albumen. It has been used hitherto in preference to any other organic compound, on account of the delicate film it forms on the paper, free from all roughness, and also on account of the beautiful color the print takes by the production of the albuminate of silver. The albumen should be used fresh, and in a slightly alkaline condition. The principal commercial objection to its employment in such a condition as the foundation of the picture, arises from the difficulty that is experienced in coating the paper evenly with it. Makers of paper prefer old albumen, which gives a slightly acid reaction. When in this last condition, the paper is easily coated, though the toning is retarded, and inferior pictures are the result.

Paper for Printing.—There are two kinds of paper used principally for albumenizing, Rives and Saxe. They are both starch-sized papers. The latter is much more porous, and consequently, less glossy than the former. Rives paper is, however, tender when wet, and tears easily when used in large pieces, such as required for large prints. Saxe, therefore, is preferred for large prints, while the Rives is admirably adapted for *small* pictures where great gloss is requisite.

The Sensitizing Bath.—The "sensitizing bath" has been a subject of dire tribulation among inexperienced photographers, or those who seem unwilling or incapacitated to receive instruction. The subjoined formula was introduced and recommended to photographers by Mr. H. O'Neil, of New York, and has been repeatedly copied and indorsed. It leaves nothing to be desired.

Silver solution, 35 grs. strong	½ gallon.
Muriatic acid	½ ounce.

Shake well, and add enough ammonia to make it slightly alkaline; shake well, filter, and use the filter over and over again. Every time you strengthen, add a little acid and ammonia. Add a little C. P. nitric acid when red teardrops appear. Fume with strong ammonia eight or ten minutes.

Preparing the Paper.—The method of floating the paper surely needs no particular description. It is by no means imperative that a certain corner should be first approached to the solution. The photographer soon acquires peculiarities of manipulation that enable him to do his work better and with more satisfaction to himself than if he followed a prescribed rule. There is one general principle to be observed. It is, that if the negatives are very hard the strength of the silver solution can be lessened, and that if they are weak, it should be much increased.

Printing and its Results.—After removal from the silver solution the paper is allowed to become thoroughly dry, and is subjected to the fumes of strong ammonia. It is then ready, when cut into the required sizes, to be placed in contact with negatives in printing frames.

The print should have the highest lights *nearly* white, and the shadows verging on a bronzed color, before toning.

Small white spots, with a black central pin-point, are often met with in prints. Dust on the paper, during sensitizing, will cause them, the dirt forming a nucleus for a minute bubble. All paper should be thoroughly dusted before being floated on the sensitizing bath.

Gray, star-like spots arise from small particles of inorganic matter, such as oxide of iron, lime, etc., which are present in the paper. They become more apparent by decomposition during the printing operations. They may generally be discernible by examining the paper by transmitted light.

Bronzed lines (straight) occur through a stoppage during floating the paper in the sensitizing solution. Should the lines be irregular, forming angles and curves, it is probable that a scum of oxide of silver, etc., may be detected on the surface of the sensitizing solution. A strip of blotting-paper drawn across the bath will remove the cause of the defect.

Should the print appear marbled, it may be surmised that the sensitizing solution is weak, or that the paper has not been floated long enough. In some cases it may arise from imperfect albumenizing, but, in ordinary commercial samples, the cause can be easily traced.

Toning.—The object of "toning" a print is to change the reduced silver salt to a sightly color that will not be destroyed, after it has been immersed in the fixing-bath. The action of toning may be considered somewhat analogous to that of intensifying the negative, by change of color; the reduction of metallic gold, from the chlorine on certain portions of the print, being similar to that of the metallic silver from the nitrate. The position of the portion of the picture on to which it is thrown down is determined by the position of the reduced silver on the paper. Where there is metallic silver, there the metallic gold is thrown down The process might be almost called "electro-gilding." The terchloride of gold, or a double salt of the terchloride of gold and potassium or sodium, is invariably used for the toning-bath, as it is necessary that the electro-gilding action should take place with a salt of gold *in solution*. It is also found advantageous that the solution should be neutral, *i. e.*, neither acid nor alkaline, the reduction taking place more rapidly than with an acid solution. The deposition of gold is further aided by the addition of an acetate or carbonate of an alkali, to form oxychloride of gold. When the terchloride of gold alone is reduced, chlorine is liberated, which attacks the silver in the print, forming fresh chloride of silver. That this action does occur may be shown by the diminished depth of color the prints assume in the toning-bath. The formation of an oxychloride of gold in the solution, however, somewhat reduces this change, a larger deposit of gold being thrown in a shorter time than if the addition of the carbonate had been omitted.

The list of well known and much used toning-baths could be made to be a very long one. They resemble each other more or less in character. There is one for which the following excellences are claimed: Beauty and permanency of tone; simplicity of formula; great economy; certainty and regularity.

The bath is made as follows. Always keep in stock the following solutions:

Solution No. 1.—Dissolve a quarter of a drachm of chloride of gold in fifteen ounces of water.

Solution No. 2.—Dissolve a quarter of a pound of acetate of soda in forty-eight ounces of water.

To make the bath, take of water thirty ounces; then add solution No. 1, three ounces; and next add solution No. 2, three ounces. Let stand a whole week before using; if wanted sooner, make it with hot water.

This bath will tone day after day until at least four sheets have been toned, and when apparently exhausted, throw away six or ten ounces of it, and add a similar quantity of fresh bath, made according to the same formula, taking care its age is not less than one week, as the acetate bath goes on improving, and if used too new would tone unevenly, and the prints would lack that brilliancy so easily obtained when the bath is of the proper age.

Always take the prints out when of a *purplish* brown, but never at the *rusty* brown stage.

If the washing has been carefully done, you will find that nearly all of the batch will be finished about the same time, vignetted portraits first, and then the plain portraits, which latter always take up a larger proportion of gold.

When the toning is finished pour your bath back into the jug or bottle, and keep the same for next time. Should there be a slight deposit of chloride at the bottom, decant carefully, so as not to disturb it; this will save all filterings, which are always better avoided.

As a rule the following will answer: "Tone the prints as you wish them to be when done."

According to the minuteness of the grains of gold, so will it assume, by reflected light, colors varying from purple to that of the ordinary yellow. The organo-chloride of silver appears through this layer of gold, and the colors of the two mingling together give the different tones in ordinary prints. When a print is not toned it becomes blue; this is due to a greater amount of gold deposited over the surface of the silver. The change in color, on the immersion of a print in the fixing-bath, is due to the solubility of the chloride of silver.

Fixing the Image.—Hyposulphite of soda is almost invariably used for fixing. A strong fixing bath is recommended, on the grounds that a double hyposulphite of soda and silver is formed, and that this double salt is soluble in hyposulphite of soda. Consequently, if enough hyposulphite of soda be added only to form the double salt in the paper, the fixing is imperfect; while an excess of hyposulphite will dissolve it out of the paper, and leave the print amenable to washing. On these grounds the strength of the fixing-bath has been made as follows:

Hyposulphite of soda	4 ounces.
Water	1 pint.

[*Mem.*—One ounce of hyposulphite of soda will fix *with safety three sheets of paper.*]

Between toning and fixing, it is well to wash the prints slightly. After taking them out of the toning-bath they should be placed in a dish of water, face downward, till a bath is ready for fixing.

It will be noticed that the toning action on the print continues during this washing, presumably by the solution of gold contained in the pores of the paper continuing to deposit. The addition of a small quantity of common salt has been found useful to stop this action If this precaution be not taken, the prints first toned should be left redder than it is intended they should remain.

The prints should be immersed in the fixing-bath for twelve or fifteen minutes. The solution should be kept in motion during the whole time of fixing, as for toning. Care should be taken to brush off all bubbles that may cling to their surfaces, as the cushion of air impedes the acess of the liquid to the silver salt.

When the prints are fixed they will appear colorless in the whites, and free from red patches in the dark portions.

In some establishments it has been found advantageous to add a drachm of ammonia to each pint of fixing solution. The ammonia aids the rapidity of fixing; it also attacks the size of the paper, dissolving it out from the paper in a great measure. This renders the washing more perfect, and is found to prevent "blistering," which is common with so many albumenized papers.

The prints should be withdrawn slowly from the bath, in order that all excess of the hyposulphite solution may be drawn from them by capillary attraction, and placed in a trough of water. The methods of eliminating the soda, or of washing the prints, must depend upon the resources of the photographer.

Prints on Plain Paper.—Prints on plain paper are useful in certain instances. The formula for preparation is given:

Chloride of ammonium	60 to 80 grs.
Citrate of soda	100 grains.
Chloride of sodium	20 to 30 grs.
Distilled water	10 ounces.
Gelatin	10 grains.

Or,

Chloride of ammonium	100 grains.
Gelatin	10 "
Water	10 ounces.

The gelatin is first dissolved in hot water, and the remaining components of the formulæ are added; it is then

filtered, and the paper is floated for three minutes. If it be required to print on plain paper in a hurry, a wash of citric acid and water (one grain to the ounce) may be brushed over the back of ordinary albumenized paper, and, when dried, the back of the paper may be sensitized and printed in the ordinary manner. For cold tones, the wash of the citric acid may be omitted.

Porcelain Printing.—Porcelain printing has been more extensively and successfully practiced in the United States than in any other country. Lately, however, the production of these pictures has been somewhat on the decline, owing primarily to the difficulty in procuring the plates for the purpose, and again, to the growing conviction among photographers that they are unstable in character, and notwithstanding the care used in their manufacture, the period of their so-called permanency is exceedingly uncertain. A most excellent formula has already been given in a previous series of "scraps" for the preparation of the collodio-chloride. Its method of use is simple, extreme care and cleanliness being the most important requirement for success.

The porcelain plates are readily cleansed with a solution of cyanide of potassium. After thorough washing, they are albumenized with a solution, one part of albumen to four parts of water. This should be flowed over the plates two or three times. When dry, they are ready for sensitizing. This is accomplished by flowing the porcelain with collodio-chloride, as one would coat a plate for negative purposes. This must be done in the dark room. The plate, while still moist, is suspended in the fuming box for two or three minutes, and then dried with a gentle heat.

Special frames are supplied by the stock-dealers for holding the porcelain in its place against the negative during the time of making the impression. The toning, fixing, and washing are to be conducted on the principles which govern the making of paper prints. The solutions, however, are to be used very much weaker than in the latter case. Fifteen minutes' washing under a moderate stream of running water will be found sufficient. There are few peculiarities of manipulation to be observed, and even these will suggest themselves with some practice of the art.

Permanent Pigment Printing.—If gelatine be mixed with a solution of chromic acid, and dried in non-actinic light, it will be found that it is perfectly soluble in water. If, however, it be exposed to the action of light, it will be found to have become insoluble. On this rests the whole superstructure of permanent pigment printing, photo-lithography, heliotypy, and processes akin to them. We will describe that which is known as the "autotype."

The manufacture of the pigmented sheets of paper upon which the pictures are printed is controlled by parties holding protective patents. The pigment consists of gelatin, sugar, soap, and colors of every tint. These are the foundation of all their permanent prints.

The tissue is sensitized by floating it on a solution of bichromate of potash and water.

Pure bichromate of potash	1 ounce.
Water	20 ounces.

The bichromate of potash should be nearly neutral, and contain no free acid. A dish somewhat larger than the paper to be floated is used. The solution should be at least a quarter of an inch in depth in the dish. The piece of pigmented paper is taken, and a quarter of an inch folded back at one end at right angles, and rolled up to a diameter of about two to three inches, gelatin surface outside. The turned-up end remains on the outside of the roll. The angle of the folded end is now dropped upon the solution, and the coil of paper is allowed to unfold itself, driving out all bubbles behind as its surface comes in contact with the solution.

The floating should last from two minutes in warm weather to three in cold. The turned-up end should then be pinned by a couple of pins on a thin lath, and slowly withdrawn from the back and hung up to dry.

The drying of the tissue should take place in a room perfectly free from vapors. If possible, a current of warm, dry air should be created through the drying room. The quicker the paper dries, the better it will work, though the less sensitive it is to light.

When quite dry the paper is exposed under the negative in the ordinary manner, a "safe edge," as it is technically termed, being placed around it. The safe edge consists of a mask of brown or other non-actinic paper, externally larger than the negative, and internally slightly smaller, the negative being, as it were, framed by it. The pigmented paper must be slightly larger (say half an inch each way) than the size of the print required. If the print be examined during exposure it will be seen that, owing to the colors added, there is no change in its appearance; consequently it is necessary to use a "photometer" to time the exposure. Considerable judgment must be used, governed by the density of the negatives to be printed from. Practice, and not directions, is here essential. When fully exposed the tissue is withdrawn from the frame in a room in which the light is weak or non-actinic. Close at hand, on a table, should be a dish containing water to the depth of an inch or more. To the bottom of this is sunk a finely mulled flat zinc plate, at least one inch larger each way than the negative; the paper is now drawn, face downward, under the water, till it nearly rests upon the zinc plate. It will be noticed that paper at first tends to coil downwards, but gradually unrolls till it is perfectly flat, and if left would coil upwards. At the moment it has become flat the zinc plate is seized by the hands, and raised horizontally from the dish, the tissue resting upon it. It is then placed on a low stool standing in another dish; one end of the paper is next pressed on to the zinc plate by one hand, and with the other the remaining portions are brought into contact with the "squeegee."

The zinc plates are termed the "temporary supports" of the tissue. They are mulled in the ordinary manner, with a muller and fine sand; the finer the grain given, the finer in detail will be the resulting pictures. They are also coated with a fatty and resinous substance, as follows: Take a piece of fine flannel, and with it rub on the plate a small quantity of this composition:

Beeswax	3 drachms.
Yellow resin	3 "
Oil of turpentine	1 pint.

With another piece polish off the excess of grease, leaving but a minute layer of it.

The zinc plates are cleaned, after use, by rubbing with flannel in boiling water. A little turpentine or ammonia may also be needed.

Development is best effected by a trough or tin basin containing water, whose temperature can be maintained at 100° Fah. by aid of a gas jet or spirit lamp. After the pigmented paper has been placed into contact by the squeegee with the zinc plate, it should be laid aside for a couple of minutes to allow the gelatin to swell. By the swelling of the gelatin a partial vacuum is created between it and the zinc plate, and the pressure of the air outside prevents it from peeling or stripping off. The zinc plate, with the adhering paper, is next placed horizontally in the trough for a minute, when it will be found that the paper can be peeled off, leaving the gelatin pigment on the zinc plate. The plate is now moved vertically in the water, and gradually those parts of the gelatin which have been unacted upon by light will dissolve away, leaving the picture beautifully developed. When the water flows from the plate free from coloring matter, it should be withdrawn and placed for a few seconds in alum and water (a dessert spoonful of alum to a couple of gallons); this renders the remaining gelatin perfectly insoluble. The plate with the picture on it should be well washed and set in a rack to dry.

The print will be found reversed. The retransfer brings it into position. The transfer paper is coated with a preparation of insoluble gelatin. The retransfer on to paper is effected in a similar manner to the transfer of the pigmented paper to the zinc. The paper is plunged into water of a temperature of 170°, where it remains till it becomes slimy to the touch. The plate bearing the dried picture is now dipped into cold water, and carries as much as possible away with it in a horizontal position on to the stool. The transfer paper is then placed, prepared side down, upon the cushion of water, and "squeezed" as before. It is then allowed to dry spontaneously (in the sun if possible), after which it will be found to readily leave the plate, bearing with it the picture on its surface.

If a mat surface be required, the print may be finished by rubbing it with cotton-wool holding a little turpentine. A brilliant surface can be given by using an encaustic paste as for silver prints:

White wax	1 ounce.
Benzole	1 "

Or,

White wax	1 ounce.
Oil of turpentine	1 "

Dissolved by the aid of heat.

Carbon prints can be produced by transferring them direct to the paper on which they are to finally rest. The method, however, necessitates a reversed negative, and that is the great drawback to the general use of the process.—*Philadelphia Photographer.*

COPYING OIL PAINTINGS.

The copying of pictures, and, *par excellence*, of oil paintings, is a branch of photography which all beyond a select few, not even excepting professional artists, look upon with distrust, not to say aversion. It is the work generally deferred till the day comes when it must be done or declined; and when it is done the result, in a great many cases, is little better than worthless. Yet there is no real need for an oil painting to be looked upon as such a bugbear; for, by the exercise of a little time and care, combined with a thoughtful appreciation of the actual requirements of the case, the occasions are not many when a photographer of average ability cannot make a fairly satisfactory copy of any picture likely to be brought to him.

We have purposely employed the word "time" as an element in the success of the photographer; for those who have not tried the production of a satisfactory negative of, for instance, an eight-foot picture, have no idea of the time taken in the fixing up alone. Every painter—at least one of any pretensions—has in his studio a large, commodious, and complete piece of machinery called a "studio easel," and with it he can readily raise or lower his canvas, place it at any angle—sideways or forwards—and, in fact, adjust it in any direction he sees fit.

But in the photographer's studio, except he be one of the few who lay themselves out for this class of work, no such thing can be found; and the time, trouble, and constant worry of shifting and altering can only be understood by those who have undergone it, on account of the absence of every likely contrivance to enable the operator to fix the picture in a suitable position with regard to light. So much is this the case that we know a professional photographer who makes a point of charging an extra fee of one guinea—that is, a guinea beyond his usual charge for this size—for taking a photograph of any oil painting brought to him. We shall mainly speak of the precautions to be taken in photographing such works when the painting can be brought to the photographer's own premises or studio.

It will be found that a large camera is difficult enough to move without any frame, and that if it be attempted, "save the trouble of taking it out of the frame," the labor will be increased tenfold; hence we would say never receive a painting until the frame has been removed, unless it be very small in size. The picture having arrived, the first step is to place it in position opposite the camera—the arrangement of lighting will be described hereafter—and for this purpose one of the cheap easels procurable from artists' colormen will be most serviceable. A very useful portable one can be had for a little above half-a-sovereign. When large framework has to be dealt with the photographer's own mother wit must be trusted to—in either case the point to receive attention being to see that the canvas is quite square to the lens. If this be not done—though it is not so absolutely essential as when copying a map or other object with many lines—considerable trouble will be experienced afterwards; for in any case a portion of the picture would have to be sacrificed to enable the print to be trimmed square for mounting.

The next point is the lighting. Here let us say that we have not found the slightest practical use from the employment of any of the commonly recommended specialties, such as colored glass before the lens, colored lighting, etc., etc. If the operator will very carefully examine a picture so placed with the usual arrangement of lighting for a sitter, he will observe patches of white light moving about in the picture as his own position alters, and, as he moves, their position in the picture will also change. If he go to one side they perhaps disappear, or if he look from a higher or lower standpoint the same thing will be observed. These spots or patches of light are reflections of the skylight and of surrounding objects; and if not removed will photograph with far more intensity than the picture itself—in fact, quite obliterate it. What, then, is required to cause such reflections to disappear?

In the first place, all light objects that are in the way must be removed. Naturally the perfection of result would be obtained if everything in the room were dead black; but, as this is impracticable, the nearest attainable approximation to it must be obtained. Light-blue studio walls are greatly to be deprecated, as it is next to impossible to avoid their appearance in some parts of the picture where least desired; and it must ever be recollected that any reflection of light objects will impress the film with greater power than the picture itself, and so swamp everything in that portion of the picture—making it look ridiculous, we might almost say. It is no use attempting to get any copy till the lighting is so arranged as to eliminate all such reflections.

It is well to call to mind the old and well-known rule as to reflecting objects—"that the angle of incidence is equal to the angle of reflection;" from which it will be seen that the nearer the illuminating area lies towards the edge of the picture, the more is its reflection thrown away from the lens. Thus, assuming, for example, that the picture were illuminated by an electric light fixed (say) at the top of the camera, no copy could be got, as the light itself would be imaged in the camera by its reflection from the polished surface of the painting at a spot a little above the level of the lens. If the light were fixed a little higher the reflection into the lens would take place from a spot a little higher up, and so on; by gradually raising the light the spot where the reflection came from would be raised higher and higher, till at last the limit would be reached and no reflection into the lens would take place, and the perfection of illuminating, so far as this phase is concerned, would be gained; but the same effect would, of course, be obtained by lateral reflection.

This is an important matter, as upon it depends almost the whole success of the photographing, and we have dwelt thus fully upon it because if once mastered in principle the power to overcome the remaining obstacles will almost follow as a matter of course. It has taken up so much of our space that we shall defer the conclusion of our instructions till next number.—*British Journal of Photography.*

PHOTOGRAPHY OF OIL PAINTINGS.

A correspondent of the *Wochenblatt* gives the following description, from personal observation, of the way in which photographic reproductions of oil paintings are made at MM. Goupil's establishment at Asnières: "New oil paintings are laid perfectly flat upon a table, and then receive the thinnest possible coating of white of egg, well beaten up, laid on with a broad beaver brush; while old or very dark pictures, or such as have mat spots, are rubbed with glycerin to bring out the details. The photograph is usually taken in the open air, on a wide terrace, or in a large courtyard, and large pictures are surrounded by large screens, made of a light wood, with dark calico stretched across, in order to keep off all reflected light. An opening is made in one of the screens, which is placed exactly opposite the picture, through which the lens, a very actinic Hermagis, may be pushed. Pictures with clear skies are canted a little forward toward the lens, or else they are turned upside down. When the picture possesses very little actinic color it must be exposed from four to seven minutes. The collodion used, which is extremely sensitive, consists of iodide of potassium (ground very fine), iodide of cadmium, bromide of zinc, and bromide of ammonium. The silver bath consists of—

Nitrate of silver	7	grammes.
Distilled water	100	"
Crystallized nitrate of zinc	3	"

In winter there is no zinc, but then there ought to be eight grammes of silver. M. Quinet himself sensitizes the large plates with eminent skill. The negatives are seldom retouched, and even in the most difficult cases as little retouch as possible is put on."

ZINC BROMIDE.

Zinc bromide, one of the most recently introduced of the haloids now used in the manufacture of collodion, possesses certain properties of its own which render it peculiarly valuable for many purposes in photography; but it is, unfortunately, a comparatively expensive salt, and its manufacture, especially to the amateur chemist, is beset by so many difficulties that it scarcely secures the amount of recognition due to it. We have to record a simple method by which it may be prepared in a state of sufficient purity for photographic use without the expenditure of any serious amount of trouble, and in the absence of any special manipulatory skill. The method is based upon the fact that cadmium is precipitated from solutions of its salts by contact with metallic zinc. All that is necessary is to make a concentrated alcoholic solution of the comparatively common and in expensive bromide of cadmium and to throw into it a few pieces of granulated zinc, leaving the solution in contact with the zinc for a day or two, with an occasional shaking to dislodge the precipitated cadmium which clings in the state of fine powder to the surface of the zinc. When the action is judged to be complete the alcoholic solution of zinc bromide is filtered in order to remove the minute particles of metallic cadmium, and may then be used without any further treatment in the preparation of the collodion. It will, of course, be necessary to form an estimate of the strength of the solution, which may be effected by actually testing a measured quantity, or, better still, a known quantity of the cadmium salt may be operated upon at first. Thus, if we dissolve two hundred and fifty-six grains of anhydrous bromide of cadmium in one ounce of alcohol (or, rather, if the bulk be made up to one ounce after the salt is dissolved), each drachm after treatment with zinc will be equivalent to forty grains of silver nitrate. The results attained in this manner will be found at least as accurate as if the crystallized salt were employed, as the latter always contains, even when freshly prepared, a large proportion of insoluble basic salt, whence it is evident that any calculations based upon the actual weight of material used must, of necessity, be open to some uncertainty.—*British Journal of Photography.*

CALIFORNIA SALT MANUFACTURE.

The Bay of San Francisco is not only one of the finest harbors in the world, but also contains in its lagoons some of the best natural salt ponds. Here a natural product, more or less impure, was found by the Indians and early settlers, sufficient for their needs. Not until a dozen years ago was any attempt made to secure a merchantable article: and then the ignorance and carelessness of the manufacturers resulted in a marked deterioration in the quality of the product. More recently the work has fallen into better hands, and now between ten and eleven thousand tons of good quality are annually produced. The mode of preparation is chiefly by evaporation, as in the making of the celebrated French Bay salt, but considerably improved both in cleanliness and in extent of circulation. A decided advantage over Eastern and European manufactures is secured by the absence of rain during the making season, thus insuring more rapid evaporation and obviating the need of sheds to cover the vats. The salt works are situated on Rock

Island, about twenty miles south of San Francisco, and about a mile from Mount Eden on the South Pacific R. R. The mode of manufacture is thus described by the San Francisco *Bulletin:*

The water is let into a large reservoir at high tide and passed along from one to another, and so on till the water has traversed a distance of 15 or 20 miles. During the time it has been flowing it has been increasing in strength and leaving limy impurities behind. In the fourth reservoir the specific gravity is 16 degrees, and leaves the seventh when it is 25 degrees. Pure chloride of sodium begins to form when the brine attains this density and continues to do so until it has attained 29 degrees. During the strengthening of the sea water from its natural specific gravity of about 1·03 to 25, the sulphate of lime held in solution crystallizes and settles to the bottom. It is not until a specific gravity of 29 is reached that chloride and sulphate of magnesia, bromide of soda and chloride of potassium begin to concrete. These being the principal, if not the only impurities, with the exception of a little water, the manner of securing pure salt appears very simple.

When at 25 degrees the pickle is run into crystallizing ponds or vats, some of which are simply the earth hollowed for the purpose, others are boarded on the bottom, while a third are made wholly of boards. In these the brine is allowed to remain until it is 28½ degrees, when the salt which has formed in the bottom is shoveled into baskets, laden on cars and conveyed to another part of the island nearer the wharf, where it is piled up in great pyramidal shaped heaps, which suggest at first sight in the distance those wonderful structures near the banks of the Nile, said to be monuments over the last resting places of departed Egyptian kings. These mounds remain exposed to the sun and weather for a year, which whitens and purifies the crystals preparatory to grinding.

The principal markets for California salt are the States and Territories along the Pacific coast, Idaho, Montana, Alaska, and British Columbia. The source of supply besides Rock Island are the lagoons of Alameda, Santa Monica, San Diego Bay and Carmen Island, on the Lower California coast, in all of which the process of manipulation is similar to that described.

IMPROVED GAS REGULATOR FOR BURNERS.

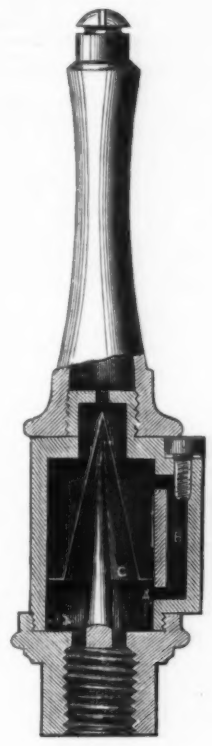
MR. D. BRUCE PEEBLES (Edinburgh) read the following paper recently before the North British Association, which we take from the *Journal of Gas Lighting:*

If we visit the seat of any of our manufacturing industries, and have the privilege of seeing the machinery employed, and watching the various processes connected with almost any kind of business, we can scarcely fail to note the extraordinary economy which pervades a well-conducted establishment. Indeed, it is now seldom that we see success attend the efforts even of the cleverest, if economy be lost sight of. The truth of the proverb, "A penny saved is a penny gained," is exemplified in every direction in which we turn, and the stimulating effect of honest competition is noticeable as being of all things the best for inducing economical efforts, and leading us to gather up the fragments so that nothing may be lost.

In gas manufacture, economy may be said to present itself in three different forms—first, in getting the greatest yield of gas out of a ton of coal with a minimum of working expenses; secondly, in utilizing all the by-products, so that as much as possible may be made out of them; and thirdly, in the using of the manufactured article in the best and most economical manner.

Under the first head may be included the mechanical stoker of Mr. Foulis, the Aitken & Young process, Mr. Hislop's new process for regenerating spent lime, and, generally, every mechanical and chemical improvement tending to cheapen the manufacture.

Under the second head may be classed those scientific discoveries from which spring the application to industrial uses of the residual products of the distillation of coal, such as naphtha, benzole, sulphate of ammonia, the aniline dyes, etc., and as a notable instance of the rise in value of residual products from these causes I may, in passing, call your attention to what appeared only lately in the pages of the *Journal of Gas Lighting*, in reference to Bradford. Between



seven and eight years ago the old Bradford Gas Company agreed, by contract, to sell the whole of their ammoniacal liquor for £800 a year. A short time ago the contract expired, and instead of £800 the corporation were offered £10,600 a year for it.

Under the third head we may class improvements in the distributary apparatus, such as gasholders, valves, pipes, fittings of all kinds, governors, burners, reflectors, and every appliance by which the gas may be distributed and consumed with the greatest economy. In connection with this part of the subject the general public are equally interested with the gas engineer, because not only economy but health has to be considered, as we all know that gas which is not properly consumed is not only wasted, but vitiates the atmosphere, and renders our dwellings unhealthy.

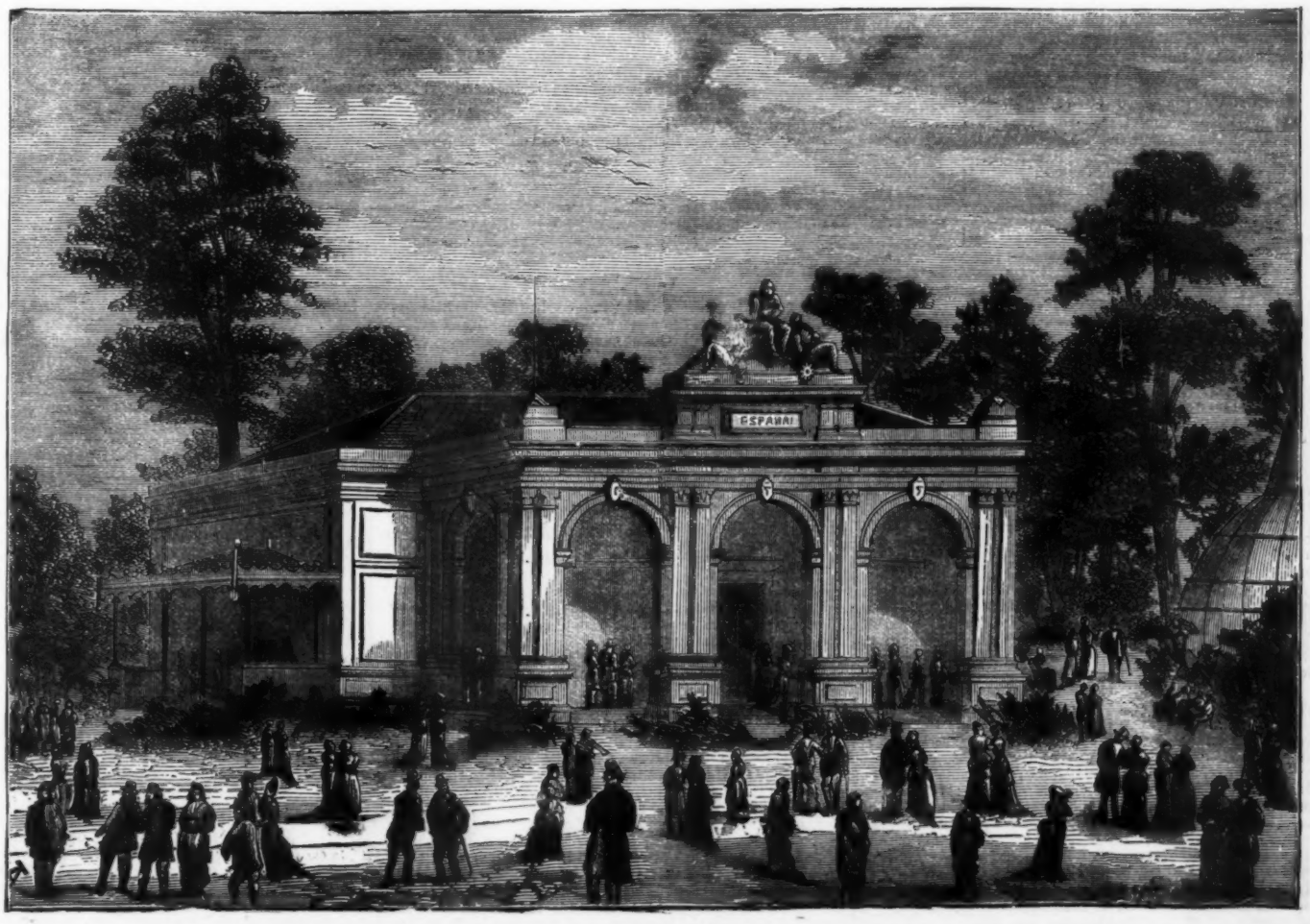
Gas managers are now looking into this branch of the subject much more than they used to do, and the popular lectures which are frequently given by them show that they are really anxious to let the public know the best methods of consuming gas; that it is not pleasant for them to make a good article and see it wasted, even though they be paid for it, and that gas economy should not be considered by them only within the gates of the gasworks, but should have their attention from retort to burner. It will be a good thing for the public when the supervision of the gas manager is extended to the internal fittings, on to, and including, the burner. That this will come, I have no doubt, and then the "gas burner man," whom we all know so well, will vanish from the scene, and bad fittings and jumping lights will no more be known.

It cannot be doubted that there is an enormous annual waste from leakage, bad burners, and excessive pressure, and it is to this part of the subject of gas economy—viz., the controlling of pressure—that I have been applying myself for a number of years past.

From the circumstance that I have long been largely engaged in the manufacture of gas governors, including those for public lamps, in which, of course, the governor is required for controlling the consumption of gas at a single burner only, I often have been asked by friends if I could not make some reliable and yet simple lamp governor, in the construction and use of which leather and liquid could be dispensed with; and, after many experiments, I believe I have succeeded in doing so. To-day I have the privilege of showing you the results of my labors in that direction, and you will observe from the drawing that the lamp governor I now bring before you is of the simplest construction.

It consists of a hollow flanged cone, resting on a needle-pointed stud, and working in a cylinder to which the flange of the cone is accurately fitted. The needle point is set exactly under the valve seat, and keeps the apex of the cone always in position, so that the valve cannot, by any means, get out of its proper place; while at the same time the greatest freedom of action is allowed to the cone. The simplicity of the apparatus is such that there seems to be scarcely a possibility of it getting out of order, and the only bad thing about it, from a manufacturer's point of view, is, that it will last forever. You will observe from the drawing that there are two lateral openings, A A, in the case, one below and another above the flange of the cone. These are connected by a vertical passage, B, and it is through this passage that the gas is supplied to the burner when a varying consumption is required, either for lamps, or for stoves, or gas fires; the apparatus is made larger when applied to the latter. A small conical-pointed screw is used to open or close the top lateral passage, the use of this screw being to vary or adjust the quantity of gas delivered at the burner. By simply turning the screw, any modification in the size of the flame or flow of gas can be readily obtained. When the size of flame and flow of gas are required to be constant, as in public lamps, the best and simplest arrangement is to dispense with these passages, and pierce a small hole, C, in the hollow cone. The size of the hole regulates the flow of gas, which, once being adjusted, a constant and unvarying discharge may be depended on.

The action of the instrument is as follows:—As soon as the stopcock is opened, the gas fills the interior of the cone, and momentarily closes the valve; but, finding its way



THE SPANISH PAVILION AT THE PARIS EXHIBITION.

the vertical passage, or through the hole, C, in the cone, it reaches the chamber above the cone. The cone is, therefore, now surrounded by gas at the same pressure, and, having nothing to support it, falls, and lets gas pass to the burner. But this only takes place to an extent that allows a differential pressure to be established, sufficient to support the cone, which is then equilibrated between two pressures, and the difference between these two pressures remains constant, however much the initial pressure of the gas may vary, unless, of course, it gets so low as not to be able to raise the cone. It follows, therefore, that a constant flow of gas will be maintained under varying pressures, and, although larger burners be used, no more gas will be allowed to pass than what the governor has been adjusted to deliver. Whenever the initial pressure of the gas is increased, there will be a greater difference of pressure between the gas below the cone and that above it, and the cone will at once rise until the equilibrium is again established. Should the initial pressure be decreased, there will be less pressure below the cone, which will at once fall, and again restore the equilibrium, so that the valve is opened by a decrease and closed by an increase of the initial pressure, by this action establishing a constant flow of gas under every change of the initial pressure, and that notwithstanding the size of the burner be changed. It must be understood that when a larger burner is employed, the pressure at the point of ignition is diminished, and *vice versa*, so that to get the best results a burner of a size suitable to give the desired pressure should be used, or the regulating screw or hole adjusted to suit any particular burner which may be wished. But with burners obtained from any good maker there will be no perceptible difference of the size of the flame, and no difference at all in the consumption, although the burners be changed, if the same number of burner be put in, which is not the case when the ordinary leather diaphragm governor is used. It is this peculiarity which forms the characteristic difference between this class of governor and the well-known flexible diaphragm governor, which I have so long manufactured. With the leather diaphragm governor, a constant pressure is maintained, with a varying volume or discharge if the size of the burner be changed. With this new governor a constant volume or discharge is maintained, with a varying pressure if the size of the burner be changed. This makes it especially valuable to gas companies who supply gas to public lamps and stair-lights by contract, at a fixed charge. Both are liable to be tampered with, either by putting on larger burners, or by widening the holes of burners which may be so securely fixed that they cannot be taken out readily. I may here explain in regard to stair-lights that our English friends will scarcely appreciate the meaning of such a term, and in case the remarks now made may not be understood by those south of the Tweed, I may simply state what you all so well know—that "stair-lights" are peculiar to the Scotch system of building dwelling-houses in flats, which is now being introduced into some districts of London, and the "stair-lights" are for the general benefit of the in-dwellers of each tenement, the gas used being paid for by them collectively. No meter being used, there is every inducement to burn the gas longer than the hours stipulated for in the contract with the gas company, and also to put on larger burners than those upon which the contract for lighting is based.

It now remains for me to put the governor in action, and show you that it does all I have said it can do; and by contrasting it with one of my ordinary lamp governors, you will be able at once to see the difference of their behavior, when tried under the same conditions.

You will notice that when I subject the ordinary governor to different initial pressures, the flame keeps steady and without variation. The gas is consumed at a certain pressure, which does not vary, whatever variations of pressure may take place in the main supply-pipe; but, if I take out the burner, and put in one of a larger size, we at once get a much larger flame, but still burning at the same pressure as before.

If I now take one of the new governors, and subject it to the same test, we observe that the flame remains constant, with every variation of the initial pressure, the same as with the ordinary governor; but if I put in a larger burner, we do not get a larger flame, as in the former case, but we get exactly the same volume of gas issuing at a lower pressure.

There is nothing new in the principle of this instrument. It was discovered long ago by Leslie, and, independently, I believe, afterward by Giroud. Since then there have been many varieties of this kind of governor manufactured, but none has been so successful as Giroud's Glycerine Rheometer, which most of you have, no doubt, seen and tested.

It would have afforded me much pleasure to have given an account of a few of the best of these inventions, and to have explained them by the help of drawings; but, as I had no intention of being here to-day as a speaker, until I lately heard from our respected Secretary that he was much in want of papers, so little time was left at my disposal that I had to abandon the idea.

I hope, however, at some future time to go more fully into the subject, and to show also some further improvements in station and district governors.

Mr. Hislop: We are frequently startled at the announcement of inventions which promise to revolutionize the whole system of artificial illumination, but we fail to realize them; we have merely the announcement that some great thing has been discovered. Here, however, we have an instrument for the regulation of gas pressure, which, I am bound to say, will revolutionize our system of public lighting. This instrument I have carefully submitted to the most severe tests that I could conceive, both as to the quantity of gas and pressure, and I could find no appreciable variation. Engineers have long expressed the desire to have an instrument that would give the same quantity of gas with different sizes of burners. To attain this has hitherto been the great difficulty. My experience of Mr. Peebles' governors has been that, while they are sent out at the beginning of the year all properly adjusted, accidents happen to the burners; the lamplighter takes them out, and for them substitutes others, and we generally find that those which have been substituted are not in favor of corporations; then, besides, the oil from the diaphragm gets down upon the valve, interferes with its action, and the mechanism thus becomes choked. This new governor seems to meet all requirements for public lighting. No longer will the surreptitious user of gas be enabled to get an increased quantity for the money he is paying; and if he substitutes a larger burner than that provided, he will perhaps have a slightly diminished quantity instead of the quantity he might expect. In this instrument we see the work of a philosophic mind and true mechanician. The instrument is unique in itself, of very small bulk, and altogether beautiful. It will, I have no doubt, meet all the requirements of those who use gaslight, and it will obviate the use of meters in the future. The instrument is a "thing of beauty," and I believe that it will be "a joy forever."

Mr. McGilchrist: The paper which we have just listened to by Mr. Peebles is, like all his former productions, very concise, and characterized by great ability. I may state that Mr. Peebles sent me one of his governors some time ago, with a view to testing its qualities, and giving the results of these tests to this meeting. I had the pleasure of a visit from Mr. Young of Clippens and Mr. Smith of Helensburgh. We all tested the governor, and on comparing notes we found that the instrument performed its work in substantially the following manner:

With the Burner.

With a pressure of 10-10ths, consumption at the rate of 2·76 feet per hour.
" " 30-10ths, " " " 2·70 "
" " 30-10ths, " " " 2·70 "
" " 40-10ths, " " " 2·70 "

Without the Burner.

With a pressure of 10-10ths, consumption at the rate of 2·73 feet per hour.
" " 40-10ths, " " " 2·55 "

You will observe that, without the burner, at 4 inches of pressure there was really less consumption than when burning under a pressure of 10-10ths with the burner. That clearly proves that the instrument is capable of doing its work well. And it is constructed of one of the most durable materials that we have, and will last for a very long time. Ultimately, I believe, it will take the place of the average meter system. This is a flow-regulator, which is said to consume a certain quantity of gas per hour, with a certain standard illuminating power. If the gas company reduces the illuminating power, a greater quantity of gas will be consumed, and, *vice versa*, if the company manufacture gas with a higher illuminating power than that at which the standard of the average governor is set, they will get the benefit in having a decreased consumption in consequence of the increase in the illuminating power. I have no doubt Mr. Peebles' governor will become very famous, and will supersede other governors in which leather is used.

Mr. McCrae: I would not be doing justice to Mr. Peebles if I did not say a word. I have devoted many years to regulators, and lamp regulators especially, and with some degree of success, and I may say if I had had the one which I now see before me, I never would have had a leather one. That is putting it shortly. I have never in all my experience seen anything approaching this instrument so suited to the purpose for which it has been made. I have never seen the instrument till to-day, and I have never had the pleasure of testing it; but it is bound to give results similar to those which we have seen and heard read. And supposing I had not seen or heard anything else to-day, I should have felt my visit to St. Andrew's largely rewarded in witnessing the experiments with this pretty little instrument, and in listening to a paper so full of instruction. I have great pleasure in expressing my approval of the instrument; and I trust Mr. Peebles will be largely rewarded for the great thought, consideration, and expense which must have been the cost of bringing out such a perfect little instrument.

Mr. Young: I have scarcely anything to add, with this exception—that Mr. McGilchrist gave the whole three of us the credit of being at the testing at the one time. Such was not the case, because I made the experiments, so far as I was concerned, personally, and some days before Mr. McGilchrist and Mr. Smith. It was some days afterward that we compared results, and I was very pleased to find they were substantially identical. I submitted the instrument to the most severe tests, and, do my best, could not get it to vary, either with or without the burner. I may remark that I am thoroughly convinced that the principle on which this instrument is constructed is the most perfect principle possible. Nine or ten months ago I had a conversation with Mr. McGilchrist, and a similar idea was then suggested to us, the only difference being that, instead of having a little cone with an orifice at the cap, I proposed to use a flat disk with a hole in the orifice, working in a cylinder. I think there is not the least doubt that this will supersede all modes of regulating the quantity to be consumed by street lamps and stair-lights. The instrument has, further, this advantage, that it is not only a mere governor with a given sized burner, but a flow-regulator, which prevents any variation in the consumption, whether you use a larger or smaller burner. The apparatus is so essentially correct in its mechanical details that I believe it will be reduced to even less space than that which it now occupies. If the flow-measurer could be reduced in size by constructing it in the form of a burner with a movable disk, such as I have just indicated, I have not the least doubt that it would be very extensively employed, and with very great advantage, by parties having various flats.

Mr. Macpherson: I feel greatly indebted to Mr. Peebles for his ingenious little instrument, and I have no doubt it will be found to be of great advantage to gas companies generally. We are considerably indebted to Mr. Peebles for many improvements, and perhaps he might add to our obligations by favoring us with a little instrument that will record the number of hours that a lamp burns—that would start when the gas was turned on, and stop when it was turned off, and then we would have the exact volume and the exact number of hours, and thus we could arrive at a perfect calculation of the quantity of gas consumed in public lighting.

IMPROVED CARBONIZING APPARATUS.

At the recent meeting of the British Association of Gas Managers, Mr. W. T. Carpenter (Sheerness) read a paper on "Improvements in Carbonizing Apparatus." The subject of improvements in carbonizing is one that every gas engineer is more or less interested in; and although much has been said and written on the subject of mechanical stoking where machinery has been made to take the place of manual labor, I am not aware that a paper has been read touching upon any of the points relative to the apparatus hereafter to to be explained.

It is not necessary that I should give a full description of all the retorts and ovens that have been designed and patented, used and in use, and the various processes for carbonizing coal for the manufacture of illuminating gas. Such a course would occupy too much time. I, therefore, intend only to treat upon those methods which may be necessary for reference. Many persons besides gas managers have been engaged on this subject, and have had fancies for novelties, upon which large sums have been spent in the construction of apparatus; to convert coals into illuminating gas, and likewise reduce the cost of production.

In the earliest days of gas manufacture the attention of engineers was given to the importance of economy in the retort house, with the view of reducing the heavy, hot, and exhaustive duties of the stokers engaged in drawing and charging the retorts. With this object, Clegg introduced his discharging and revolving web, on which the coals were made to pass through the retorts by mean of a flat iron chain; and although it is recorded that excellent results were obtained both in quantity and quality of illuminating gas, the scheme was abandoned on account of the destructive nature of the hot coke on the iron chain. This system was continuous, and came very near to perfect carbonization.

A Mr. Hobbins, of Walsall, also patented a system of through retorts, where a series of rakes was used to level the coals and push out the coke, but, as with Clegg's revolving web, the heated coke destroyed the rake, and, not finding it so economical as was expected, it was abandoned.

We have it also recorded that Mr. Brunton, when engineer of the Birmingham Gas Works, had a discharging system of retorts, whereby the coals were thrust into the retort, the coke ultimately falling into a water cistern, from whence it was collected. The inventor had 50 retorts at work on this system at one time, and claimed an increase of 35 per cent. in volume, also a saving in labor. The important increase said to have been obtained by these pioneers in gas manufacture must not be too lightly passed over. Had these statements been made at the present time, with clay retorts worked at high heats and with improved exhausting machinery, the surprise would not be so great; but we may infer that, as iron retorts were used in both Clegg's and Brunton's systems, there are some reliable data for the inquiring gas engineer to work upon, and we have, as recorded, certain facts which lead us to believe that the last-named processes were only partial failures. We, therefore, find there is still room for improvement in the retort house, for the stokers have yet to perform their duties. It is true, machines and improved stoking tools have been introduced and placed in the stoker's hands, but the work of drawing and charging the retorts has still to be performed.

The advantage to be derived by the use of machinery is obvious, and, whether in gas works or any other manufactory, its introduction is with a view of reducing the heavy labor and cost of production. How far this can be carried out in the retort house is a matter yet to be solved, and it behooves the gas engineer to introduce machinery wherever it is practicable.

I have for many years directed my attention to the subject of improvements, and have constructed an apparatus which promises to go far toward reducing the heavy labor of the stoker, having the novelty of being nearly self-acting and continuous, having gravity for its principle, giving equal results with less labor, and accomplishing the most disagreeable part of a stoker's duty.

The apparatus consists of a double bench of retorts and oven combined, the retorts being open at the back end, and communicating with the oven into which the coke is discharged, and where, after remaining some time, it is allowed to fall out.

The engravings on the next page show a front elevation, side elevation, and transverse section. A is a hopper, into which the coals are placed; B is a pulley; C is a vertical shaft, with arms, to keep the coals from clogging; and D is a worm on the driving-shaft. E is the mouth-piece, and F F are wheels which give motion to the screws or regulators, X X. H H are the retorts, and I I are the openings pierced in the retorts for the exit of gas to ascension-pipe. J is the oven into which the coke is discharged; K is the main flue which heats the oven, and L and N are eduction-pipes for carrying off the gas. M is the coke outlet, into which is fitted a door, opening downward.

The action of the apparatus is as follows: Coals are placed in the hoppers and descend into the mouth-piece, and, falling on the screws, are gradually pushed into the retort, becoming soft by means of the heat, and forming themselves into the shape of the retort. They thus completely fill the small end and keep back the gas; and as the coals are screwed into the retort the charge assumes a shape, so as to half fill the interior area, the gas finding its escape by the outlet, I. The motion is continuous, and the charge reaching the end of the retort becomes coke of a fragile nature, falls into the oven, where any gas remaining in it is driven off, and escapes from the top of the oven by the pipe, L. After the coke has remained in the oven some time it is allowed to fall out by opening the door for that purpose. The door is then immediately closed in readiness for the next charge.

The apparatus having been practically at work at different times, opportunity occurred for testing its merits. The trials were conducted in the following manner: All other retorts were struck and drawn and the valves shut, the index of the station-meter taken, and the hoppers filled with coals. When the apparatus had been at work for the time appointed the index of the station-meter was again taken, coals weighed to refill the hoppers, and the coke discharged and carefully measured.

Trial No. 1.—Time eight hours, exhauster worked at $\frac{3}{10}$ths pressure in the retorts, the hydraulic dip dispensed with, and by-pass in use. Coals carbonized, 12 cwts.; gas registered, 6,210 cubic feet, or 10,350 cubic feet per ton; coke discharged, 21 bushels; illuminating power, 13½ candles. When this trial took place the apparatus had been at work 29 days.

Trial No. 2.—Time eight hours, exhauster worked $\frac{2}{10}$ths pressure in the retorts. Coals carbonized, 9½ cwts.; gas registered, 5,200 cubic feet, or 10,947 cubic feet per ton; coke discharged, 18 bushels; illuminating power, 14 candles.

Pelaw Main coal was the description used.

There is a specialty in connection with this apparatus, to which I beg to call your attention, viz., the principle on which it is constructed—that is, gravity; for on the coals being once placed in the hoppers, which can be done by means of a Jacob's ladder or elevator at a trifling cost, they are kept continually moving until they reach the oven. This moving causes the gas to escape from the interior of the charge, greatly facilitating the carbonization of the coals.

It is not necessary to crush the coals, as this must be regulated by the pitch of the screw. For instance, a 4-inch pitch will take a piece of coal the same size, and in well-regulated gas works no larger pieces should be used in any process. Should it happen that a larger piece finds its way to the screw, it will get crushed by it as it revolves, or it slips the screw out of action. This circumstance will be seen by the man in charge, but it seldom happens, and is soon remedied.

Again, the manufacture of coal gas, under any process, deposits a layer of carbon in the retorts, and to remove this carbon is generally a difficult task, and not a few retorts are injured and destroyed by the process. If not injured by the chipping bars the retorts are allowed to stand open, exposed to the atmosphere for several days, in order to burn it off. Should it be necessary to clean the carbon off in this setting, the front plugs are taken out of the mouth-pieces.

and the charge pushed down into the oven and discharged; the door is then left open, when a strong upward current of air passes through each retort, and the carbon is effectually removed in a few hours.

I have simply stated these facts to elucidate, if possible, the advantages of the system.

In conclusion, I may state that the apparatus is erected at the end of the retort house—a great distance from the engine—motion being transmitted by means of ordinary chains and pulleys. Very little power is required to work the setting.

To better illustrate the system, there is a model, and a screw which has been used during the time the setting has been in action, both of which you may inspect, also a sample of coke.

[The discussion which follows has reference to the paper, by Mr. G. E. Stevenson, on "Some Notes on the Construction of Retort-Settings," published in last week's *Journal*, as well as to the above paper.]

Mr. Barratt (Grantham) said there were several questions required to be asked with regard to Mr. Stevenson's setting. This was the very life of their system. They either lived by a proper application of their retorts or died by the abuse of them; and it was essentially necessary to ascertain whether there was any advantage in this method or not. He thought a great deal would depend on the quality of the coal used; and their Scotch brethren had an advantage here, because they had a better quality of coke from the north country coal, which would evolve a greater quantity of heat than any other coal. If these retorts could be set in such a way that they would last for a considerable time with a minimum quantity of coal per ton carbonized, they would be in a position to judge of the benefit of the system, and no one could confer more benefit on the Association than by bringing out the best system of setting retorts.

Mr. Gore believed that every disease required its own remedy; and so every class of coal would require a special system of treatment, as the system which was perfectly applicable to one class might be perfectly inapplicable to another. He thought it was from this fact that they had such conflicting statements made as to the results obtained. It was quite certain that all the coal fields of England produced coal which might manufacture light-giving gas, but the quantity of it would vary, to a great extent depending on the position of the stratification of coal and the source from which it was obtained. Suppose they had a retort-setting of that class and had to deal with some of the cannel coals of Scotland, the necessary consequence would be that, instead of getting from 10,000 to 11,000 feet per ton, which a Scotch manager would expect to get, he would probably get no more than 7,000 or 8,000 feet, simply because the amount of heat necessary to evolve the gas from its cells in the coal was not supplied rapidly enough. Again, they might be in a district where the gas was the product most required, and where they could afford to expend an extra bushel or two of coke with advantage. It was quite certain that a furnace which would consume a large quantity of coke, and give out a greater quantity of heat in a given time, was more applicable to such a district than one with slower combustion, less product of heat, and, consequently, a smaller result during that given time. In judging, therefore, the value of any system of retort-setting, they must be guided by a variety of circumstances, and thus the engineers of each locality must select for themselves. He was satisfied that if they would deal with this matter on a different footing from what they had hitherto done—if they abandoned, to a certain extent, the "follow-my-leader" principle in using any system, and were only guided by the lessons it taught—they would gain great advantage. Mr. Carpenter had read a most valuable paper on continuous carbonization, and there were circumstances where such a system might be of inestimable value. It seemed that the coke produced from such a system would be of little value; but there might be places where coke was only of secondary importance, and the light evolved of great importance. So that the two systems, although diametrically opposite to each other, would have advantages which were well worthy of attention.

Mr. West (Maidstone) desired to thank Mr. Carpenter for the long time and attention he had given to the subject. He had spent a great deal of time in working out what he thought would be the most perfect system of carbonization, and he was not in a position to say it was not so, though he was to some extent opposed to him, still he thought he was on the right tack. Whatever invention was brought forward, the most important work of a gas manager was that of carbonizing coal in the retort. Mr. Carpenter had spent a great deal of time and money on this matter, and anything he had said was worthy of great consideration. He referred to the pressure on the retorts, and that was a very important principle. In many London and large provincial works they worked with a vacuum, and he thought the right principle was to keep the pressure in the retorts as light as possible, say an eighth or a quarter of an inch. The question of retort-setting was no doubt an important one, and bore on the manufacture of retorts. Some men might have retorts which would not last half the time that they did with others. How was that? Because one might work with a vacuum and the other with pressure. If one worked under the least possible pressure, he thereby not only got the volume of gas from the coal, but the full illuminating power. With regard to Mr. Carpenter's apparatus, the questions of cost and wear and tear would arise. No doubt the best retorts were often put into arches and destroyed in a short time, and the manufacturer was sometimes blamed, when in all probability he did not deserve it. He held that they should work under pressure, and see that the furnaces were properly kept up, and then no doubt, as a rule, they would get more gas and a higher illuminating power.

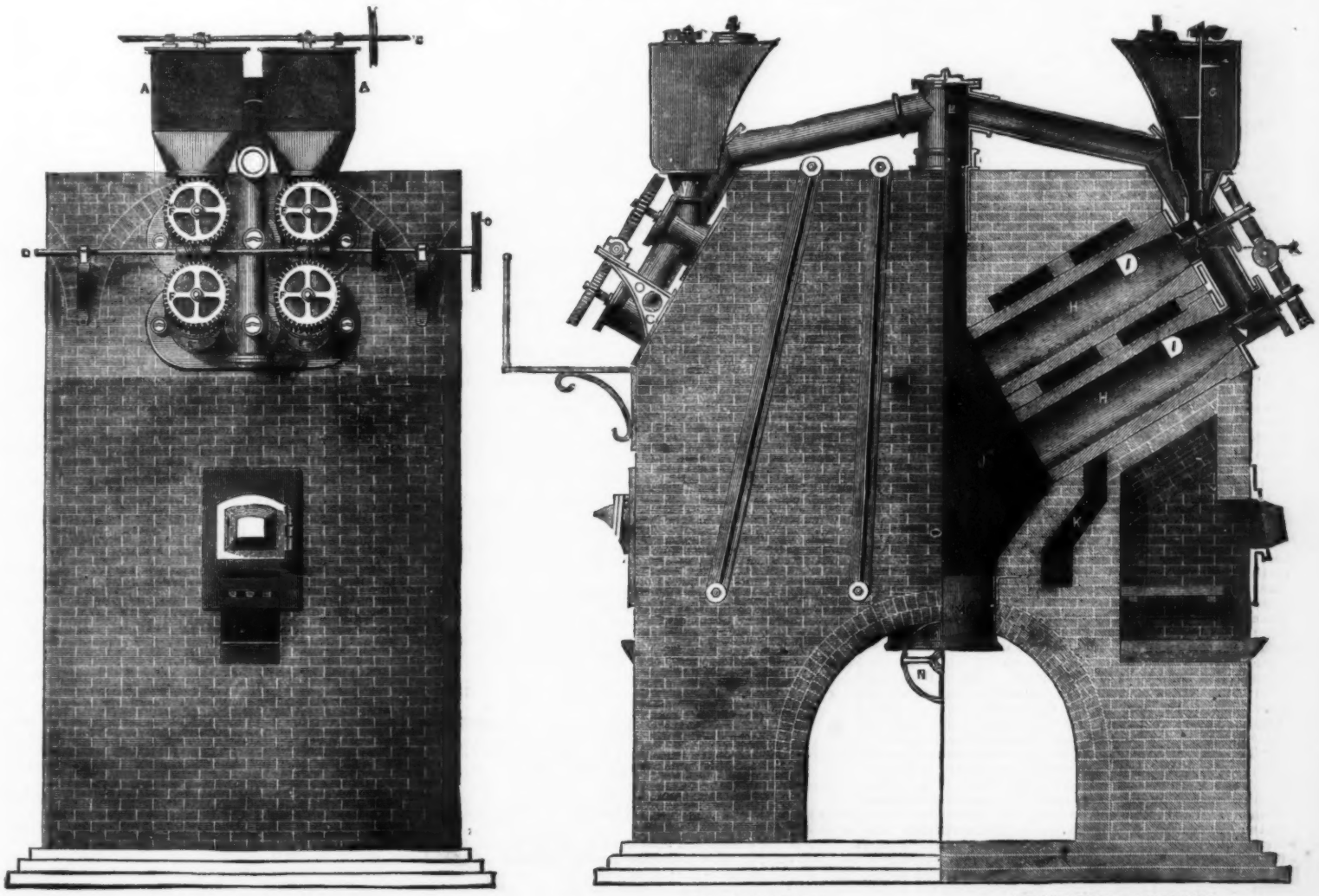
Mr. Stevenson said he was pleased that the remarks which had been made were of a nature to widen the field and call for considerable attention. He desired to express his great pleasure at hearing of the new method brought forward by Mr. Carpenter, which he believed was a thoroughly good thing. It reminded him of the invention of Messrs. Porter and Lane, who proposed to carbonize coal in retorts by means of a screw. They set their retorts vertically, and experiments were made at the Horseferry Road station; but there they failed, because the screw, which was of cast iron, in several pieces, would not stand the heat. Apart from that the principle was sound and the idea good. In Mr. Carpenter's arrangement that evil was remedied.

Mr. Wilson said a few years ago he patented an invention very much the same.

Mr. Stevenson said, with regard to his own paper, the first speaker referred to the seven retorts as not being so good an arrangement as the six, and as far as the combustion of fuel was concerned and the dropping out of the retort, he quite agreed with him. For many reasons he preferred the setting of six, but his settings were arranged for seven before and he thought his best plan was to do what he could, with the seven. There was economy of space in this plan and the middle retort did not in this system, to any appreciable extent, diminish the heat of the others. He had not attempted to introduce atmospheric air above the furnace doors to consume the carbonic oxide that might be formed, but relied entirely on the air space in the furnace, and he believed that no carbonic oxide left it unconsumed in any form. The draught was not great, and there was plenty of time for the gases to be converted into carbonic acid. With regard to the class of coal used, no doubt different coals gave different results; but he could not understand what Mr. Gore said about his setting not being able to carbonize Scotch coals. He was not acquainted with Scotch coals other than the Scotch cannels; but in ordinary settings they could carbonize cannels as well as ordinary coal, and it was continuously so used. His aim had been to get a higher heat, not to reduce it; for he did not think it possible to go to too high a heat, having regard to the illuminating power of the gas It might be a question whether it was desirable to use cannel to get a certain illuminating power; but the better and quicker it did it, the more economical it was. He used Silkstone coal, but he believed the setting would give equally good results with Newcastle. With regard to the pressure, he used an ordinary dip-pipe set to 1¼ inch. He drew 1 inch vacuum, so that there was half an inch pressure in the retorts; and as the connections and main were of ample size, he did not think that the pressure was any more than that shown on the gauges.

Mr. Carpenter, in reply, said there was no opposition between him and Mr. West. If that gentleman would only employ a steam engine to work his apparatus, instead of manual labor, no better system could be produced. He believed the apparatus he had described could be erected for £25 per mouthpiece from the foundation to the top. Of course the elevator or hoisting apparatus would do for a whole retort-house. With reference to Mr. Gore's remarks about a continuous pressure, so well experienced as he was he knew well the different systems, and it would be useless for him to expatiate with advantage on the saving of labor if they could be carried out. If the drawing and charging could be done away with, the saving in labor would be about 40 per cent. He had seen results from gas-works capable of making 60 to 70 million cubic feet per year, and he believed that his system, or one like it, whether carried out by himself or any one else, would be a great benefit to the gas manufacturer, simply because the work of the stoker was a degrading duty.

The President said they would all feel that Mr. Stevenson's paper was an admirable one, being an effort to bring into system the observations of a careful, observant man. If they could get something more in the shape of definite data to guide them in the apportionment of their apparatus throughout, it would be a very great help, and save every man watching and determining for himself the proportion of the different parts of gas-works. One object of the Association should be to bring together the observations of each man, so as to bring to something like system the apportionment of apparatus, and other matters of the same sort. As to Mr. Carpenter, of course he would simply say that it had nothing whatever to do with processes that might have been patented or invented in times gone by. He was perfectly sure that he had been laboring throughout on an idea which had occurred to him as an original idea, and the paper was eminently creditable to him.—*Journal of Gas Lighting.*



FRONT ELEVATION. SIDE ELEVATION. TRANSVERSE SECTION.

IMPROVED CARBONIZING APPARATUS FOR GAS MANUFACTURE.

REPORT ON THE RECENT ECLIPSE AS SEEN IN THE UNITED STATES, BY THE ASTRONOMER ROYAL OF GREAT BRITAIN.

THE ECLIPSE.*

The telegram sent from Denver, signed by Professors Young and Watson, Dr. Draper and myself, will have given an idea of the results which we think have been secured on the eclipse of this year. Since the telegram was dispatched I have been engaged in making as many notes on the various points of detail as incessant travel and a temperature of 91° in the shade would permit. A long time must elapse before anything like a general view of the total work done is possible, but I think that the readers of *Nature* may rely upon the correctness of what I have collected, though it is quite possible, as I have not succeeded in finding all the observers, that some errors may have crept in. And again, it is quite possible that the many dry plates taken and even yet not developed may contain information of which at present we have no idea. To take an instance: we do not yet know whether Prof. Harkness' attempt to photograph the polariscopic phenomena presented by the corona was successful or not, for I learn at the Naval Observatory here that he is still at Fort Steele, handing over his camp equipment to the military, who all along the line have been placed in the most unreserved way at the disposal of the different parties by the express orders of Gen. Sherman himself, who takes the greatest interest in such inquiries, and is anxious to foster scientific inquiries as far as in him lies. Strange as it may seem, this is the expressed feeling of all the authorities here, from the Chief of the State downward. In interviews with which I have been honored, the President of the United States himself, the Secretary of War, Gen. Sherman, and other members of the Cabinet, have one and all insisted upon the importance of securing records of all possible natural phenomena, and expressed their gratification that such records have been secured in the present instance by Government aid.

I will begin the extracts from my notes by referring to the appearance of the Corona itself.

I give a rough sketch of what *I* saw of the corona with the naked eye (Fig. 1), slightly exaggerating the dimensions of the streamers to show the wind-vane appearance, which, to me, was almost perfect, being pointed at one end and bounded by parallel edges at the other; others, I may say, however, saw a resemblance to a fish's tail. These streamers vanished absolutely in the telescope (Fig. 2), as did the radiating lines in 1871; not a shred of them was left. Prof. Cleveland Abbe (lying on his back at a height of eleven feet on Pike's Peak; he had been sent down the day before from the summit, as the rarefaction of the air was too much for him) saw them with the naked eye extending to a distance of six degrees on either side, and their appearance

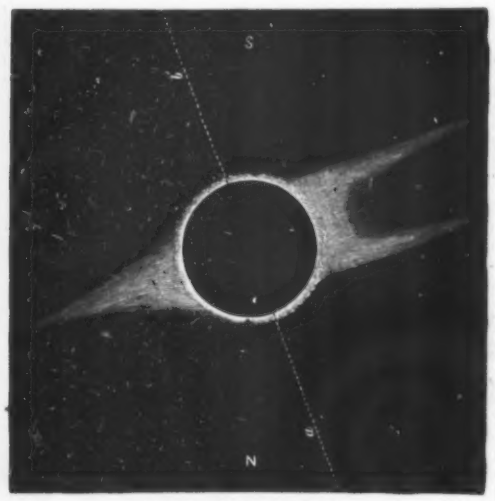


Fig. 1.—NAKED EYE.

suggested to him meteoric streams for their origin. Prof. Newcomb also saw them with the naked eye when he had hidden the corona behind a screen. To him they suggested the zodiacal light, or rather its nucleus near the sun, even if it extend beyond the orbit of the earth.

These streamers seem to have been seen by everybody, and were doubtless cosmical; a system at right angles to them (they lay along the ecliptic), and quite as bright, was recorded by many, though many of the best observers saw not a trace of them.

Here is a lesson, and one which affords an explanation of a great deal of eclipse work connected with these outliers of the corona.

I had a magnificent view of the corona with a power of 50 on my 3¾-inch Cooke, and saw exquisite structure at the north and south points. Curves of contrary flexure started thence, and turned over, and blended with the rest of the corona, which was entirely structureless and cloud-like; the filamentous tracery which in India I observed till three minutes after totality had ended had all gone. Prof. Bass, however, tells me that by confining his attention to the same point for nearly the whole of totality, the structure came out, and seemed to pulsate like an aurora.

Prof. Hall is almost the only one who is under the impression that the corona of 1869 was less brilliant than this.

Mr. Burnham, who is an observer of the highest order, thus gives his opinion, which agrees with that of Prof. Young, who remarked its unusual faintness and lack of polar extension, and all the other American astronomers:

"The coronal display was far less than in the eclipse of 1869, as seen at Des Moines, Iowa, by members of the *Times* party, and it bore a more striking resemblance, probably, to the eclipse of 1870, as seen at Gibraltar and in the Island of Sicily. The corona was, in fact, a mild affair, according to the observations of this party, as compared with that seen in other eclipses. A few protuberances were seen, and several bright streamers. On one side there was observed a bright pink ribbon or crescent. The traditional bright lines (the rays) and dark patches (the rifts) were not nearly as conspicuous as usual."

Mr. Burnham made another interesting observation which may prove one of considerable value in determining the nature of the corona. Anticipating that the star ζ Cancri would be very near to the sun, he made special efforts to see it, and was altogether successful, for it was distinctly seen through the corona.

General Myer, the distinguished head of the Army Signal Service, who had given orders to utilize his station on Pike's Peak for eclipse observations, observed the corona

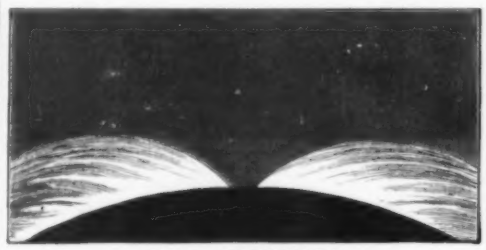


Fig. 2.—TELESCOPE.

himself from the summit, and therefore under conditions which have never been utilized before. He describes the corona as built up of five radial lines of a golden color: beyond this in the direction of the ecliptic were prolonged bright silver rays. This was seen with the naked eye. In the telescope the appearance was quite different; a layer close to the sun, only of a light pink color, was seen, and the long bright silver rays had disappeared. The greater elevation, thus, was more suited to a study of the structure than the lower levels, and at the same time the color observed seems to have been slightly changed. In the pure air of the peak, also, he saw the corona steadily for about five minutes after totality, and watched the moon cover the outer striated edge of the corona, which appeared then to be more golden than ever.

We next come to the Photographs.

Photographs of the corona and of its spectrum were obtained at nearly all the stations. In order to give an idea of the extensive preparations which had been made, I may state what were Dr. Draper's photographic appliances as an instance of the equipment at one station.

In the first place there was a telescope of 5 inches aperture, and 78 inches focal length, especially corrected for photography, to get as complete a photograph of the corona as could be obtained by an exposure lasting during the whole of totality. This was during 165 seconds.

Next there was a large instrument which may be called a slitless "phototelespectroscope." This instrument consisted of an object-glass, composed of four lenses 6 inches in diameter and 21 inches focal length. The image of the sun at the focus was less than 1/6 of an inch in diameter, and of extreme brilliancy. Before the rays of this lens reached the focus, however, they were intercepted by a Rutherfurd grating, about 2 inches square, set at an angle of 60 degrees. This threw the rays to one side, and produced three images—a central one of the sun, and on either side of it a spectrum. One of these spectra was dispersed twice as much as the other, that is, would give a photograph of twice the length. This last photograph was actually about 2 inches long. With this instrument, mounted equatorially and driven by clockwork, if the light of the corona was due to gas, giving lines which lay in the actinic region of the spectrum, ring-formed images, one ring for each bright line, would be introduced. On the other hand, if the light of the corona arose from solid or liquid bodies, or was reflected light from the sun, a long band answering to the actinic region of the spectrum alone would be produced. If the light was partly from gas and partly reflected sunlight, a result partly of rings and partly a band would have been obtained.

As there was an impression that it was impossible to obtain a photograph of the spectrum of the corona, Dr. Draper, in order to give the best possible chance of getting such a photograph, resorted to exceedingly sensitive materials, known as the lightning collodion process, furnished by the Messrs. Anthony, of New York. This involved a necessity of distilling a large part of the water used, because at Rawlins the water contains either alkali or sulphur, both of which are deleterious.

Since so many attempts were made, I had better confine my attention to the photographs. I have seen those of Prof. Hall and Dr. Draper. I will first deal with those of the corona itself. The former, taken by a dry plate process, and, I believe, by means of a large apertured camera of short focus, are very admirable, and show the structure at the north and south points most exquisitely.

One of Dr. Draper's was exposed during the whole of totality, and represents a corona somewhat like those seen in the photographs of the 1871 eclipse taken with a medium exposure. In another, by a fortunate shake of the instrument, a trace of the outline of the chromosphere was received.

Next I come to the Spectra; but first I may be permitted to refer to the only photographic contrivance which was possible for my light marching order. This was a small portrait camera with a lens of one inch aperture, and a Rutherfurd grating of about 6,000 lines to the inch in front of it to act as a reflector and disperser.

I placed the camera and the attached grating on the ground in such a position that on the focusing screen I got the direct image of the sun at one edge of the plate, and running across the plate the spectra of the first and second orders on one side. A rough notion of what is seen on the

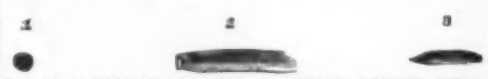


Fig. 3.—1, IMAGE OF SUN; 2, FIRST ORDER SPECTRUM; 3, SECOND ORDER.

plate when the sun is photographed thus will be gathered from Fig. 3.

Now if the corona had been built up of gases competent to give us lines with a spectroscope of ordinary construction, what I ought to get during an eclipse from this instrument would have been something like Fig. 4, *provided I had a clockwork arrangement to drive it.* Without this contrivance the image and spectra will travel along the plate in the direction of the sun's motion—let us suppose as here in Fig. 5.

It will be thus clear that, with this simple contrivance, the result obtained, if clockwork be employed, will be very similar in kind to that arrived at in Dr. Draper's more elaborate arrangement, to which attention has been drawn.

In the spectra, then, thus obtained, we had only one question to solve, as we all thought: we had to determine the position of the rings photographed in the first and second order spectra, or, at all events, in the first. Long before I developed my plate I knew what had happened. Dr. Draper, Prof. Thorpe, and myself had photographed the spectrum—this I never doubted—but we had photographed as continuous a one as if a platinum wire, or ball, in a state of incandescence, had been in front of our instrument, instead of a shell of hydrogen gas 6′ high, as in 1869, and 10′ high, as in 1871.

Dr. Draper's plate was exposed during the whole of totality, to make assurance doubly sure. The continuous spectrum has no trace of a ring, though the plate was considerably over-exposed. In my own little photograph the continuous spectrum only is shown, and very clearly. While my plate was being exposed I held a duplicate grating close to the eye, expecting to see the rings as I saw them through five prisms in 1871. In first, second, and third order it was all the same—

Continuous spectrum,
Continuous spectrum,
Continuous spectrum;

and it was this which told me of the gigantic change which had taken place in the atmosphere of the great body we were studying.

The remarkable phenomenon thus photographed was, of course, as remarked by the eye observers: still bright lines were seen in the corona spectrum by one observer, using the term corona to represent everything outside the photosphere, and I make this remark because whether the spectrum observed was that of the phenomenon which was visible or not I cannot say.

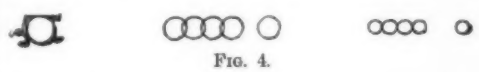


Fig. 4.

Prof. Young himself and many other observers saw the reversal of many lines at the moment of totality, thereby indorsing Pye and Maclear's observations of 1870 and 1871. After this, to all but Prof. Young the lines vanished utterly, and this was so absolute that in three parties there was consternation and fear, when nothing but a continuous spectrum was seen, that something had gone wrong with the instruments.

Prof. Young saw 1474 very faint, *F without C*, and, strangest of all, the H and K lines without the long calcium line. This is one of the most remarkable results of the eclipse, and must set students of solar physics to work on a new line. Young thinks the H and K lines were brilliant enough to suggest that the ring in this part of the spectrum photographed in the eclipse of 1875 was really due to them, and I am inclined to agree with him, though why the H and K lines should appear minus the longest line of calcium passes comprehension, unless calcium is dissociated there from some cause of which at present we have no idea.

The discovery of a line in the ultra red by means of a thermopile is another victory gained, and I sincerely hope that at the next eclipse Capt. Abney may use some "red molecules" to photograph it. Mr. Edison, with his wonderful tasimeter, also found indications of heat in the corona, but as he did not use a spectrum, he did not imitate Prof. Young in determining the exact position of the radiation.

We next come to the Dark Lines observed in the corona spectrum.

Janssen was the first to announce the fact that in the corona spectrum the chief Fraunhofer lines were to be seen. This observation was made on the Indian eclipse of 1871. The natural interpretation of this observation is that in the coronal atmosphere there are molecules sufficiently complex to reflect rays of every refrangibility, and that they do in reality reflect the solar light to us as it reaches them.

The observations on this point are a little doubtful, though it is held generally that the balance of evidence is in favor of their appearance. Prof. Barker, who observed with Dr. Draper at Rawlins, saw these lines at the moment that he was dumfounded, as were most of the observers, at the absence of the bright ones. On the other hand, at Prof. Newcomb's camp, at Separation, they were not traced, Com-

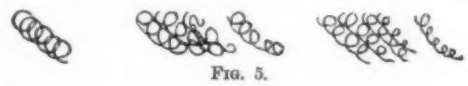


Fig. 5.

mander Sampson seeing nothing but a perfectly continuous spectrum; 1474, F, and even C, being utterly invisible. If the corona gives us light containing the Fraunhofer lines, it can only do so because it reflects solar light. Let us next see, then, what the evidence as to Polarization is.

When all the observations on this subject are collected we may hope for much new knowledge, but the matter was not finally settled this time, for here again was a surprise.

Professors Morton and Hastings had arranged to determine the quality of the polarization observed, Prof. Harkness was to attempt to photograph the phenomena, while Prof. Wright set himself the difficult task of quantifying it. One observer was specially told off to settle the questions raised by Prof. Pickering's observations of 1870. I may commence by saying that these last observations suggest that in 1870 the instrument used by Prof. Pickering was out of order, so that now everybody agrees that there is polarization.

I have not yet been able to learn anything of the results obtained by Professors Wright and Harkness, as the latter used dry plates, which were to be developed here, and the Las Animas party took a long mountain trip to shake off the bad effects of a long camping on an alkali plain, and so did not put in an appearance at Denver.

Prof. Morton got radial polarization most distinctly, as I did in India in 1871. The *amari aliquid* is to be found in Prof. Hastings' results, which will be best seen from the following account obligingly placed at my disposal by Prof. Bass, one of his party:

"The most important and unexpected result of the expedition was reached by Dr. Hastings in his polariscopic observations. It will be remembered that in previous expeditions, and by many observers in the present eclipse, only a few seconds were devoted to the examination of the corona for polarization, and this by a bi-quartz or a Savart polariscope,

* From *Nature*.

held in the hand so as to take in the whole corona in one view. In the organization of the expedition, Dr. Hastings was requested to prepare a plan by which the question as to whether the polarization of the corona was radial or tangential, or absent, could be definitively settled. This was accomplished by an arrangement of apparatus essentially novel.

"The four-inch telescope had a Savart polariscope at its eye end. A diaphragm perforated with holes, 3', 4', 5', 8', 10', 16', was interposed between the polariscope and the objective, and the polarization phenomena of a definite and limited portion of the corona were thus seen. Diaphragms of 3' and 5' were alone used. The telescope was pointed by Lieut. Very to six points, the first one above the sun and 50' from the circumference; the second one 25' from the circumference and 45° from the vertical; the third 1¼° from the sun and to the right of it; the fourth 16' from the sun and to the right of it; the fifth 16' from the sun and 135° from the vertical; the sixth 8' from the sun and 135° from the vertical. These points were selected before the eclipse, but it was not known to Dr. Hastings in what order they would be viewed, so that the readings were absolutely free from bias. The position of the pointer of the polariscope was carefully verified before and after the totality. The readings were reduced immediately, and each reading (which was, as is evident, independent of every other) agreed in showing that the plane of polarization was perpendicular to the sun's radius through the point examined—that is, *tangential*—a most unexpected result, about which, however, there is no doubt whatever. Four of the readings were made with a dark band in the center and two with a bright band. This result does not agree with others from the same eclipse, for the reason that in the method adopted by Prof. Morton, Mr. Lockyer, and others, there was, first, no means adopted for isolating a definite portion of the corona and determining its special polarization; second, that in the use of the Savart form the phenomena of radial or tangential polarization alike present bands radial to the sun; third, that, with the bi-quartz, the field is so small that it becomes extremely difficult to interpret the tints of color seen, if, indeed, any are seen. The noval fact was shown, by the use of this method, that the polarization of the corona was exceedingly strong near to the sun's limb (one and a half minutes), and was relatively weak far from it."

I believe that no one is more astonished than Prof. Hastings himself at the result of his work, which, it has been laughingly said, goes to demonstrate the existence of *ice crystals* in the corona. With reference to the remarks made on my 1871 work in the foregoing, I may add that I used a bi-quartz and a large field, so that the objections raised to the method do not apply.

This brings me to the end of my notes for the present on the photographic, spectroscopic, and polariscopic results obtained. Of course there was a whole world of wonders outside these fields of inquiry.

The view of the shadow of the moon from the summit of Pike's Peak, cleaving its way along the lower air, has been described to me as one of the most striking phenomena which it is possible to witness or imagine. The shadow, the boundary of which was seen curved, was preceded and followed by a spectrum. Where I was, nearly 8,000 feet high, and therefore a little over mid-height, there was no effect on the air, but after the close of totality the shadow was observed passing over clouds near the horizon. I may add, however, that the phenomena at the beginning and end of totality hardly existed for me, for I had to uncover and cover my photographic plates at those times.

It was not to be expected that in this country, where the anxiety for news and views seems always to be at fever heat, the astronomers would be allowed to quit their stations without giving an idea of the tendency of their work, and even its connection with the torrid temperature through which we have had to make our dusty way, or in which we have had to exist when locomotion had become impossible.

The utter disappearance of the large bright red corona of former years, in favor of a smaller and white one in this year of minimum, struck everybody. Indeed it is a remarkable thing that after all our past study of eclipses this last one should have exhibited phenomena the least anticipated. It isolates the matter that gives us the continuous spectrum from the other known gaseous constituents. The present eclipse has accomplished, if nothing else, the excellent result of intensifying our knowledge concerning the running down of the solar energy. With the reduction of the number of spots or prominences for the last four years the terrestrial magnetism has been less energetic than it has been for the preceding forty years, while at both ends of this period we have had famines in India and China.

As the sun is the great prime mover of earth, and as every cloud, every air current depends upon it, its present quiet condition is worthy of the most minute study. The absence of lines from the corona spectrum shows a great reduction in the temperature of the sun, and such a marked change in the sun should produce a corresponding change on the earth, so that the associated terrestrial phenomena should be carefully observed. Hence I regard this eclipse as the most important that has been observed for many years, as it throws much needed light on many points hitherto obscured in doubt.

Prof. Morton, of the Stevens Institute of Technology, remarked that the thing which impressed him most was the very curious character of the result: While on former occasions there has been projected into the space surrounding the sun a quantity of self-luminous gaseous matter which has no fixed place there, we now see that this was merely a temporary occupant which has, either by diffusion into space or absorption into the body of the sun, been removed. He also holds that the marked changes in the sun's condition would seem to call for corresponding marked changes in the condition of the earth. The results, in fact, recall to his mind one of the early theories of the maintenance of the sun's heat, which was that it was maintained by the impact of meteoric matter constantly falling in upon the sun from surrounding space. The quantity of heat produced by such an infall may be realized from the fact that a body so falling would develop by impact as much heat as 5,000 times its weight of carbon burnt in oxygen.

Portions of the meteoric matter in the shape of meteorites fall upon the earth, and we thus know that most of them consist largely of iron, containing considerable quantities of absorbed gases; others are of a metallic character and devoid of such gases. If, now, we suppose that for a considerable period the sun's fires were fed with hydrogen-charged fagots, and then again that the main supply was of mineral matter, we might have at first a far-reaching atmosphere of gas, such as has appeared hitherto, and at another time this might be absent, which seems to be the case at present.

He further adds that if such changes go on indefinitely it may not be irrational to inquire whether they may not in future produce such extraordinary climatic conditions in the earth as geology teaches us have existed in the ages of the past.

Professor Young was careful not to commit himself to any decided connection between solar and terrestrial climatic changes; he, however, certainly concurs with me that the corona is fainter and the gaseous elements far less conspicuous than that observed at previous eclipses, and acknowledges that the different condition of the corona proves a change in the condition of the sun, as the corona acts with it in a sort of sympathy. Dr. Draper is resolute on the other side. He is reported to have said:

"It is rather singular, while the sun has been in such a quiescent condition for more than two years, that we have not seen more changes in the climate of the earth. This would seem to show that the abnormal condition of the sun at the maximum period of sun-spots, which occurs every eleven years, counts for but little against the total amount of heat that is sent out from the sun at all times. The present observations go to show that the activity or quiescence of the sun makes no perceptible difference in the earth's condition. I do not regard this most marked change in the corona as portending any change in the condition of either climate or crops."

Finally, on this whole question, I may remark that I have not been a little astonished to find how slowly European work percolates among the men of science here. I have met few who are familiar with Meldrum's admirable work, and the discussion to which it has given rise. Still it is a great thing that at all events the cycle of solar changes has forced itself so markedly upon public attention.

For myself, as I have ever regarded sun-spots as down-rushes (a term to which I still adhere) I am well content to see this view indorsed by such a chain of facts as the corona has now supplied. In spot-maximum years we have violent up-rushes of gas from the sun's interior, and the corona is mainly built up of such gas. Further, we have spots, and, if these are not evidences of the return convection currents, we have none other. In spot-minimum years, such as the present, we have no up-rushes, and the corona contains no gas, and there are no spots. Spots, then, are only observed when we have a right to look for the return of the upward current, about which there is no doubt, and the rate of which we have measured.

But if this puts beyond all question, as I claim it does, the nature of spots, on the other hand the separation of the gaseous from the continuous spectrum of the corona indicates that we have yet much to learn of the temperature and nature of the corona when the spots are absent.

So much, then, touching the progress of solar matters during the eclipse of 1878. I have not yet, however, done with the observations.

There is little doubt, I think, that an intra-Mercurial planet has been found by Prof. Watson. If it will fit one of Leverrier's orbits, and should turn out to be Vulcan, no doubt astronomers will be able to keep a firm grasp upon it, and sooner or later its elements will be determined.

Prof. Watson, of Ann Arbor, whose belt, as the papers here put it, is graced with the scalps of I know not how many minor planets, broke off work on a planet beyond Neptune to come to discover one inside Mercury. He went with me in Mr. Silvis' railway photographic car from Rawlins to Separation on the morning of the eclipse, intending to observe with me at the station we were determined to occupy, with our light equipments, as the number of detached clouds visible at the time of totality on the previous days had strongly shown the advantage of separating the parties as much as possible. We chose a spot to leeward of one of the enormous water tanks of the Union Pacific Railway, which form the chief features in the interesting but desolate plains in that region, over which the wind sweeps at times with incredible violence.

On reaching our destination we found Prof. Newcomb, whose camp was about a mile away, and it was then agreed that as he and Prof. Watson were to hunt for the planet they had better be together, so I lost his company during the eclipse.

Prof. Watson's plan of operation was to sweep south of the sun and observe all the stars in the map, a part of which is here produced (Fig. 6), and to refer the position of any new body to the stars, or, if possible, to the sun itself. For this purpose, with the assistance of the Rawlins carpenter, he armed his equatorial with paper circles and brass wire pointers. He commenced operations to the left of the sun and saw the stars marked, but none others. Then sweeping out to the star marked *b*, he noticed on his return another not on the chart, marked *a*. He then made three marks on his right ascension paper circle, on the spots occupied by the pointer, when the sun, *a* and *b* were successively brought into the center of the field. He next determined the difference of declination in the same way between the sun and *a*, having the additional help that *a* was nearly in the same declination as *b*. He then repeated his R A. measures, and called Prof. Newcomb, but the eclipse was over before any more could be done. I give this statement from memory only (as I was too busy to make notes at the time), as I heard it soon after the eclipse at the camp, before the telescope was dismounted. It is probable that the subsequent careful measures of the circles may alter the place—

R.A. 8h. 26m.
Dec. + 18° 00'

I telegraphed to you, somewhat, but the alteration will be small.*

Since arriving here I have learned that Mr. Swift, of Rochester, a well tried observer, also saw the planet. The first account I read of his work was as follows;

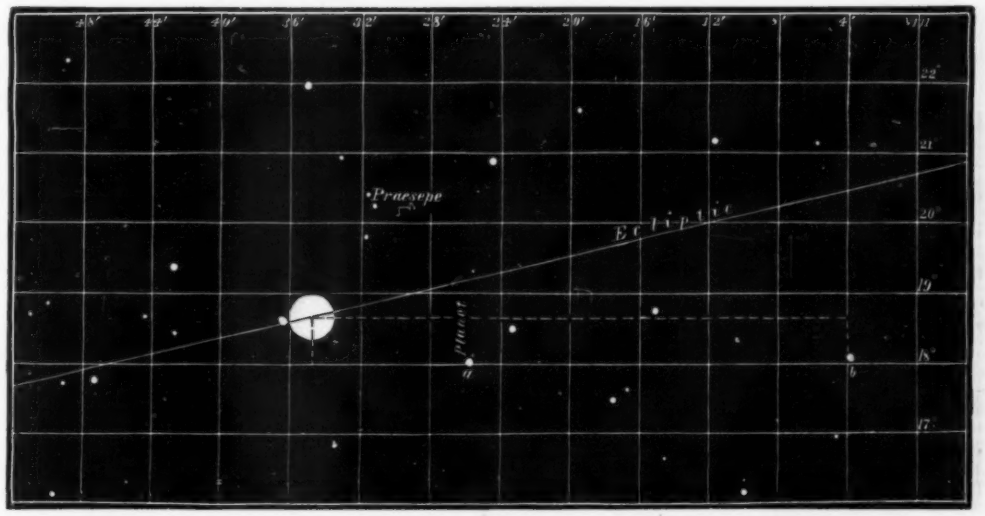


FIG. 6.—PROF. WATSON'S OBSERVATIONS.

"This gentleman made a very careful search for Vulcan, scanning the heavens very closely with his splendid comet eye-piece, made by the celebrated Gundlach, but he saw nothing of it. He did, however, see, about three degrees from the sun, two stars not down in the charts or star maps, and about as bright as the pole star—they were pointing directly toward the sun. On attempting to refind them, he was prevented by a little cloud."

Since then, however, another fuller account of his work has appeared, from which I gather that about one minute after the commencement of totality two stars caught his (Mr. Swift's) eye about three degrees, by estimation, southwest of the sun. He saw them twice, and attempted a third observation, but a small cloud obscured the locality. The stars were both of the fifth magnitude, and but one is on the chart of the heavens. This star he recognized as Theta in Cancer. The two stars were about eight minutes apart. There is no such configuration of stars in the constellation of Cancer. In 1859, the French astronomer, Lescarbault, claimed that he had seen an intra-Mercurial planet crossing the sun's disk. He related his discovery to Leverrier, who became a firm believer in the existence of such a planet. The perturbations of Mercury's orbit demand such a planet as Leverrier named Vulcan. The star Mr Swift saw may have been the same that was seen by Prof. Watson, who was located at Rawlins, Wy. T.

Mr. Swift possessed a comet eye-piece of very flat and large field, and distinct to the very edge. It was made in Rochester, and to it and his blunder in failing to untie his instrument he believes he owes his success.

The instrument used in the search for Vulcan by Prof. Holden proved to be inadequate to show all the stars on the Washington star map, owing to the brightness of the corona. The space where the planet seen by Prof. Watson was, was four times swept over, but so near to the sun as this a four and a half magnitude star was not to be seen. A space of 10° in declination by 3° in right ascension was twice swept over.

Here, again, we get an idea of the thoroughness with which the work has been planned and executed.

It would be wrong to conclude these hurried notes without stating that, from the day in which I landed in New York to the present time, I have become everybody's debtor for

* On going to press we receive a letter from Prof. Watson, dated Ann Arbor, August 14, stating that the result of more careful examination gives —Washington M. T., July 29, 5h. 10m., R. A., 8h. 20m. 54s., Dec. + 18° 10'.

acts of kindness, which have touched me greatly. This great country is a land of true courtesy for which I here express my gratitude, not only to my scientific brethren, and chiefly to Dr. Draper, General Myer, and Prof. Newcomb, whose guest I have been, but to hundreds to whom I have been a stranger and unknown.

As significant of the keen interest taken in the eclipse by all classes here, I may mention, in conclusion, that on the Sunday before the event prayers for fine weather were offered in all the churches in Denver.

J. Norman Lockyer.

Washington, D. C., August 8.

REPORT OF PROF. HENRY DRAPER, OF NEW YORK.

As I have been recently giving attention to the subject of solar spectroscopy in consequence of my discovery of oxygen in the sun, it seemed desirable to take advantage of the total eclipse of July 29, to gain as precise an idea as possible of the nature of the corona, because the study of that envelope has been regarded as impossible at other times. The main point to ascertain was whether the corona was an incandescent gas shining by its own light, or whether it shone by reflected sunlight.

For this purpose I organized an expedition, and was fortunate enough to secure the co-operation of my friends Professors Barker and Morton, and Mr. Edison. The scheme of operation was as follows: (1) the photographic and photospectroscopic work, as well as the eye slitless spectroscope, were to be in charge of my wife and myself; (2) the analyzing slit spectroscope was in charge of Prof. Barker, with the especial object of ascertaining the presence of bright lines or else of dark Fraunhofer lines in the corona; (3) the polariscopic examinations were confided to Prof. Morton, who was also to spend a few moments in looking for bright or dark lines with a hand spectroscope; (4) Mr. Edison carried with him one of his newly-invented tasimeters, with the batteries, resistance-coils, Thomson's galvanometer, etc., required to determine whether the heat of the corona could be measured.

This entire programme was successfully carried out, and good fortune attended us in every particular. The results obtained were: (1) the spectrum of the corona was photographed and shown to be of the same character as that of the sun, and not due to a special incandescent gas; (2) a fine photograph of the corona was obtained, extending in some parts to a height of more than twenty minutes of arc, that is, more than 500,000 miles; (3) the Fraunhofer dark lines were observed by both Professors Barker and Morton in the corona; (4) the polarization was shown by Prof. Morton to be such as would answer to reflected solar light; (5) Mr. Edison found that the heat of the corona was sufficient to send the indices beam of light entirely off the scale of the galvanometer. Some negative results were also reached, the principal one being that the 1474 K, or so-called corona, line was either very faint or else not present at all in the upper part of the corona, because it could not be observed with a slitless spectroscope, and the slit spectroscope only showed it close to the sun.

The general conclusion that follows from these results is that on this occasion we have ascertained the true nature of the corona, viz., it shines by light reflected from the sun by a cloud of meteors surrounding that luminary, and that on former occasions it has been infiltrated with materials thrown up from the chromosphere, notably with the 1474 matter and hydrogen. As the chromosphere is now quiescent, this infiltration has taken place to a scarcely perceptible degree recently. This explanation of the nature of the corona reconciles itself so well with many facts that have been difficult to explain, such as the low pressure at the surface of the sun, that it gains thereby additional strength.

The station occupied by my temporary observatory was Rawlins (lat. 41° 48′ 50″, long. 2h. 0m. 44s. W. of Washington, height 6,732 feet above the sea), on the line of the Union Pacific Railroad; because, while it was near the central line of totality, it had also the advantages of being supplied with water from the granite of Cherokee Mountain, and of having a repair shop, where mechanical work could be done. I knew by former experience that the air there was dry and apt to be cloudless; in this particular our anticipations were more than fulfilled by the event, for the day of totality was almost without a cloud, and the dew-point was more than 34° F. below the temperature.

The instruments we took with us were as follows, and weighed altogether almost a ton: 1st. An equatorial mounting, with spring governor driving clock, lent by Prof. Pickering, director of Harvard Observatory. 2d. A telescope of 5¼ inches aperture and 78 inches focal length, furnished with a lens specially corrected for photography, by Alvan Clark & Sons. 3d. A quadruple achromatic objective of 6 inches aperture and 21 inches focal length, lent by Messrs. E. & H. T. Anthony, of New York. To this lens was attached a Rutherfurd diffraction grating nearly 2 inches square, ruled on speculum metal. The arrangement, with its plate-holders, etc., will be designated as a phototelespectroscope. 4th. A 4-inch achromatic telescope with Merz direct vision spectroscope, brought by Prof. Barker from the collection of the University of Pennsylvania. 5th. A 4-inch achromatic telescope, also brought by Prof. Barker; to it was attached Edison's tasimeter. Besides these there were polariscopes, a grating spectroscope, an eye slitless spectroscope with 2-inch telescope, and, finally, a full set of chemicals for Anthony's lightning collodion process, which, in my experience, is fully three times quicker than any other process.

The arrangement of the phototelespectroscope requires further description, for success in the work it was intended to do, viz., photographing the diffraction spectrum of the corona, was difficult, and, in the opinion of many of my friends, impossible. In order to have every chance of success it is necessary to procure a lens of large aperture and the shortest attainable focal length, and to have a grating of the largest size adjusted in such a way as to utilize the beam of light to the best advantage. Moreover, the apparatus must be mounted equatorially and driven by clockwork, so that the exposure may last for the whole time of totality, and the photographic work must be done by the most sensitive wet process. After some experiments during the summer of 1877 and the spring of 1878, the following form was adopted:

The lens, being of 6 inches aperture and 21 inches focal length, gave an image of the sun less than ¼ of an inch in diameter and of extreme brilliancy. Before the beam of light from the lens reached a focus it was intercepted by the Rutherfurd grating set at an angle of 60 degrees. This threw the beam on one side and produced there three images—a central one of the sun, and on either side of it a spectrum; these were received on three separate sensitive plates. One of these spectra was dispersed twice as much as the other, that is, gave a photograph twice as long. Thislast photograph was actually about two inches long in the actinic region. If now the light of the corona was from incandescent gas giving bright lines which lay in the actinic region of the spectrum, I should have procured ring-formed images, one ring for each bright line. On the other hand, if the light of the corona arose from incandescent solid or liquid bodies, or was reflected light from the sun, I was certain to obtain a long band in my photograph answering to the actinic region of the spectrum. If the light was partly from gas and partly from reflected sunlight, a result partly of rings and partly a band would have appeared.

Immediately after the totality was over and on developing the photographs, I found that the spectrum photographs were continuous bands without the least trace of a ring. I was not surprised at this result, because during the totality I had the opportunity of studying the corona through a telescope arranged in substantially the same way as the phototelespectroscope, and saw no sign of a ring.

The plain photograph of the corona taken with my large equatorial (exposure 150 seconds) on this occasion shows that the corona is not arranged centrally with regard to the sun. The great mass of the matter lies in the plane of the ecliptic, but not equally distributed. To the eye it extended about a degree and a half from the sun toward the west, while it was scarcely a degree in length toward the east. The mass of meteors, if such be the construction of the corona, is therefore probably arranged in an elliptical form round the sun.

For the fortunate results of this expedition we are not a little indebted to the railroad and express companies. The Pennsylvania, the Chicago and Northwestern, and the Union Pacific railroads, the Pullman Palace Car Company, and the American and Union Pacific express companies made the most liberal arrangements, and Mr. Galbraith, the superintendent of the Repair Works at Rawlins, gave us the free use of his private house and grounds. Of the citizens of Rawlins it is only necessary to say that we never even put the lock on the door of the observatory, and not a thing was disturbed or misplaced during our ten days' residence, though we had many visitors. They sent us away with a serenade.

Henry Draper.

THE LATE TOTAL ECLIPSE OF THE SUN.

By Richard A. Proctor.

Before many days we may expect to have fuller news than has yet been received respecting the total solar eclipse of July 29th. There are those who infer from the telegrams that little of importance has been discovered on the present occasion. For my own part, I believe that we shall find that few eclipses have revealed results of greater interest. When the American photographs have been duly dealt with, and good engravings of them sent over to our scientific societies, we shall probably find the last lingering doubts respecting the solar nature of the corona definitely disposed of. I must confess I was surprised to observe that the correspondent of the *Daily News*, easily to be identified as Mr. Lockyer, still retains rather more than doubt on the subject of coronal rays. He definitely advances the startling assertion that the observations made during the eclipses of 1870 and 1871 established the fact that the rays seen extending far beyond the coronal glory are either optical, terrestrial, or cislunar, as distinguished from solar phenomena. Somewhat as he maintained that the photographs of the corona obtained by Willard at Xeres, and by Brothers at Syracuse, in December, 1871, were entirely discordant, until it was established beyond all possibility of doubt that they were in close agreement; and as, in December, 1872, while the photographs which settled the whole question were being developed, a telegram from him was on its way to Jaffna assuring the observers there (waiting then for totality) that the corona had been *proved* to be almost wholly terrestrial; so, in his letter to the *Daily News*, he stands out for the non-solar nature of the coronal rays within a few days of the eclipse, which has most probably established their solar character in a manner which even he will recognize as demonstrative. Of course I would not deny that an observer may for a moment see beams which have no existence but in his own eye, or that, at the very beginning or at the very end of total eclipse, a long ray of sunlight in our own air may seem to belong to the sun. But the idea that rays which have been seen during the whole continuance of total eclipse can be non-solar phenomena is one which no one at all acquainted with the facts (with some small inkling of mathematics to aid him in interpreting them) can entertain for a moment. I believe it had its origin in an ingenious experiment devised by the late Father Secchi, and sometimes called the "shutter-and-bung" experiment. If a round hole in a shutter is imperfectly stopped by a somewhat irregular bung, and an observer, in a room which receives no light except through this hole, looks directly at the bung, he will see a number of rays resembling in their general appearance the coronal rays seen during total eclipse. The notion suggested to some by this experiment is that the moon may perform some such part as the irregular bung. They overlook the fact that the shutter is as necessary as the bung to the shutter-and-bung experiment. In a total eclipse the shutter is omitted; and the omission is rather important. Again, it is not true that photography has failed to show the coronal rays. I have before me, as I write, a magnificent proof of the engraving representing the photographic views of the corona in December, 1871; and many straight rays can be traced to a distance corresponding to more than five hundred thousand miles from the eclipsed sun. These, of course, being photographed at the beginning, middle, and end of totality, and at two stations hundreds of miles apart (one at the sea level, the other ten thousand miles above it), are necessarily solar. The rays were seen to a much greater distance; but, unless the wild (one might almost say the insane) theory is maintained that the parts photographed were solar, while their fainter prolongations were nearer to us than the moon, the photographs taken in December, 1871, cannot honestly be said to have demonstrated the non-solar character of the coronal radiations. Nay, even on that stupendously absurd assumption, the evidence of the photographs respecting the fainter parts of the rays could only be regarded as negative. But I believe every one whose opinion on such a question could be of the least weight recognizes the photographs taken in 1870 and 1871 as demonstrating the solar nature of the whole coronal structure—though, doubtless, some faint luminosity over the coronal region is due to the illumination of our own air by the solar corona. How faint this luminosity must be is shown by the fact that over the dark body of the moon, where this light is of necessity strongest (the part of the air directly between the observer and the moon receiving more light from the corona than any other), the luminosity is so faint that to ordinary vision the moon looks absolutely black during totality.

One reason why I believe that the observations made during the recent eclipse will prove very valuable is this, that the greater number of observers were stationed high above the sea level. Much less air intervened between them and the corona than under ordinary conditions. It has always been noticed that the coronal rays are better seen the clearer the air is and the higher the observer's station. For instance, whereas the greater number of the observers of the eclipse of 1869 saw no rays, General Myer, stationed 5,530 feet above the sea level, saw four rays extending to a distance of two or three diameters of the sun—say to between 1,700,000 and 2,600,000 miles. That the conditions were favorable on July 29 last is shown by the circumstances that Professor Newcomb, employing a method of observation which I suggested some five or six years ago, was able to trace the faint light of the zodiacal to a distance of about six degrees from the eclipsed sun. This had never before been accomplished. Indeed many thought that the zodiacal light never could be seen during totality. But in the *Gentleman's Magazine* for July last I expressed the opinion, confirmed by the event, that, by screening the brighter part of the corona from view, the eye might, under favorable atmospheric conditions, detect the faint light of the zodiacal. We learn that Professor Newcomb erected a screen on a high pole, and traced the zodiacal to a distance of six degrees, or some eight million miles from the sun.

Another important discovery is the recognition of the fact that the inner corona last July differed markedly from the corona which had been seen in 1871. It was much less extensive, but much brighter. Its light was not that of glowing gas, but of glowing solid or liquid matter. Its color was not pink, but pearly white. The eclipse of 1871 occurred when the sun had many spots, whereas at present there are few spots. Accordingly, Mr. Lockyer infers that the corona sympathizes with the sun, and is expanded, faint, and partly gaseous, when the sun is greatly disturbed, while it is contracted, bright, and free from gaseity when the sun shows no spots. The generalization is daring, but scarcely trustworthy; *c'est magnifique, mais ce n'est pas la science*. It requires only a careful study of the evidence afforded by former eclipses to discover that in a year when the sun is very little disturbed, the expanded and faint corona may be seen, while in a year when the sun is greatly disturbed we may see the contracted and bright corona. There may be a connection between the corona and the sun spots. Indeed, before the present eclipse occurred, I indicated the probability that, as the prominences and sierra seemed to sympathize with the sun, so also may the corona. But the law is not so simple as has been supposed. Many eclipses must be observed before the exact nature of the law connecting the phenomena of the corona and the sun spots can be detected. I have examined in an article which will appear shortly in the *Contemporary Review* the evidence from the eclipse of 1567 to the present time; but it is not sufficient to enable us to form a satisfactory theory. Overhasty generalization is the bane of true science.

If the telegrams can be trusted, Prof. Watson of Ann Arbor, the discoverer of some score of planets traveling between Mars and Jupiter, has discovered a planet traveling between Mercury and the sun. It is announced in the *Times* telegram that a "body" between the fourth and fifth magnitudes was seen in such and such right ascension and declination during totality. I pointed out at the time—indeed I believe I was the first to point out—that the star Theta Cancri was very near the spot indicated by these telegrams. Yet (as I also mentioned) Professor Watson was not likely to mistake Theta Cancri for a planet. I have since received from Washington the chart issued for the guidance of those who were to look for an intra-Mercurial planet. I find that it includes Theta Cancri, as might have been anticipated. On the whole I think it extremely unlikely that an astronomer so skillful as Professor Watson should have mistaken Theta Cancri for a planet. I fancy he saw both the star and the stranger, and that it was by seeing the two bodies thus in juxtaposition that he was able so definitely to indicate the brightness of the unknown body. If *so*, a new planet—which *may* be Lescarbault's Vulcan—has been discovered, traveling between the sun and Mercury.—*London Echo*.

VULCAN'S YEAR.

By A. F. Goddard, Sacramento, California.

Most of us are familiar with the Vulcan excitement which sprang up about two years ago, when the late eminent Leverrier again called the attention of astronomers to the probability of a transit of Vulcan in October, which he deduced from the reported observation of Weber, in Prussia, April 4, 1876, and Dr. Lescarbault's observation of March 26, 1859, and the apparent relation or sequence of these to the catalogue of observations since 1802 and possibly 1750. The data being imperfect, however, Leverrier repeatedly changed his views regarding the apparent period of the planet's revolution round the sun. In 1859 about 20 days was the assigned period, while two years ago Leverrier varied from 42·02 days to 28 days and 33 days. No doubt Leverrier gave his views pretty fully to the French Academy of Sciences, but comparatively little explanation has appeared in the public press generally. The London *Times*, January 16, 1860, published a letter of Prof. Hind, remarking that Leverrier considered that Vulcan's transits could only occur between March 25th and April 10th, and September 28th and October 12th, in any or every year. But in 1876, although the first 10 days of October were assigned, it was still not regarded as impossible as late as October 30th, and the year 1877 astronomers were requested to look out about the 22d of March. Meanwhile, Mr. W. G. Wright, of San Bernardino, having been reading on the 23d of October, 1876, some interesting remarks in the Scientific American of October 21st, which, being published in New York in advance of date, reaches here on date, and having a 4-inch lens telescope, determined to search for himself, and next day, at about 3 P.M., was surprised and delighted at the first focus, as he expressed it, to see the planet of Vulcan distinctly before him. After calling his family to enjoy the spectacle with him, which they did, and observing in half an hour undoubted planetary movements across the solar disk, he telegraphed to Prof. Davidson at San Francisco, "Transit of Vulcan this afternoon; look for it." But the Professor, being at Washington, did not get the dispatch until long afterward. Mr. Wright then, with some rather imperfect appliances, succeeded after some trouble in getting a tolerable negative photograph of the transit, and estimated roughly that it commenced about sunrise, and would last until about 9 P.M., or about 15 hours, but that was, doubtless, an overestimate, as he afterward admitted in a letter to myself. He communicates the particulars to the Scientific American, which appeared, with illustration, November 18, 1876. That article attracted my attention al-

most more than the previous discussion, although, indeed, I had felt considerable interest in the planet since its reported discovery by Dr. Lescarbault in 1859, the merits of which, Prof. Hind said, he hoped would always be allowed to Dr. Lescarbault; and I was also surprised that more notice had not been taken of Prof. Hind's advice in 1860, "that although transits might afford the best chance for observance, still the planet very possibly might be detected in full daylight in certain parts of the orbit, provided powerful instruments were employed and proper attention given to focal adjustment." In other words, when the planet was in elongation, or at the greatest apparent distance from the sun, it might be a favorable period for observation. The first question, therefore, was, what was the probable portion of the planet's apparent revolution? I saw that Leverrier computed its probable distance from the sun at about one-fifth the earth's distance, which would make it about 19,000,000 miles, and the whole orbit would therefore be about 120,000,000 miles. I then assumed the velocity of the planet at about one-third greater than Mercury's, or about 136,000 miles per hour, and the chord of the planet's path across the sun, as shown by Mr. Wright's diagram, about 816,000 miles, being rather less than the whole solar diameter. The planet would therefore take about 6 hours crossing the sun, and its orbital year would be about 37 days; but in 37 days the earth will have moved more than one-tenth of its orbit, so that the Vulcanian apparent year or revolution would be about 40·7 days, or about 40d. 16h. 48m. But while theory might favor as much as that, still any well-reported observations should have their due weight; and I soon found that 40d. 15h. 30m. gave 5 equal revolutions between Weber's of April 4, 1876, calling the hour 9h. 30m. A.M., to Wright's, October 24th, 3h. 0m. P.M., 1877, while the same period gave 153 revolutions to Dr. Lescarbault's discovery, March 26, 1859, calling the hour 5h. 0m. P.M. This concord was extremely gratifying. My first communication to the *Alta California* was published November 21, 1876, and as further light seemed to dawn, I continued the correspondence at intervals. Subsequently I examined most of the accredited observations from 1802 to 1876, of which I had the dates. And as Leverrier had indicated that there might be some irregularity in the periods of revolution, there might be no objection to a few hours' difference. After carefully revising the whole of them, I think the following will be approximate, always allowing that, having only the days of the month of all the observations excepting Mr. Wright's, I have had to calculate the time of day, in accordance with my theory; the whole being regarded as San Bernardino time, which, I believe, is about 20 minutes earlier than Sacramento time:

EPHEMERIS OF VULCAN'S TRANSITS FROM 1802 TO 1878.

FRITSCH—October 10th, 3h. 0m. P.M., 1802; 6,209d. 6h., or 153 revolutions of 40d. 14h.

STARK—October 9th, 9h. P.M., 1819; 7,297d. 12h., or 180 revolutions of 40d. 13h.

DE CUPPI—October 2d, 9h. 15m. A.M., 1839; 7,115d. 3h. 45m., or 175 revolutions of 40d. 15h. 47m.

Dr. LESCARBAULT—March 26th, 1h. P.M., 1859; 6,218d. 19h. 30m., or 153 revolutions of 40d. 15h. 30m.

WEBER—April 4th, 8h. 30m. A.M., 1876; 203d. 5h. 30m., or 5 revolutions of 40d. 15h. 30m.

WRIGHT—October 24th, 2h. P.M., 1876; 162d. 14h., or 4 revolutions of 40d. 15h. 30m.

GODDARD'S COMPUTATION.

April 5th, 4h. A.M., 1877; 203d. 5h. 30m., or 5 revolutions of 40d. 15h. 30m.

October 25th, 9h. 30m. A.M., 1877; 162d. 14h., or 4 revolutions of 40d. 15h. 30m.

April 5th, 11h. 30m. P.M., 1878; 1 revolution of 40d. 15h. 30m.

Inferior conjunction May 16th, 3h. 0m. P.M., 1878; 1 revolution of 40d. 15h. 30m.

Inferior conjunction June 26th, 6h. 30m. A.M., 20d. 7h. 45m.

Superior conjunction July 16th, 2h. 15m. P.M., 10d. 3h. 52½m.

Elongation July 26th, 6h. 7½m. P.M.

Total eclipse of the sun July 29th, 2 P.M.

Conjunction August 5th, 10h. 0m. A.M.

Elongation August 16th, 1h. 52½m. A.M.

Superior conjunction August 26th, 5h. 45m. A.M.

Elongation September 5th, 9h. 37½m. A.M.

Conjunction September 15th, 1h. 30m. P.M.

Elongation September 25th, 5h. 22½m. P.M.

Conjunction October 5th, 9h. 15m. P.M.

Elongation October 16th, 1h. 7½m. A.M.

Conjunction, or Transit, October 26th, 5h. 0m. A.M.

—*Sacramento Valley Agriculturist.*

OUTLINES OF CHEMISTRY.

By HENRY M. MCINTIRE, M.E.

Selenium (atomic weight, 79·5; symbol, Se).—Selenium was discovered by Berzelius in 1817, and by him named. (Selenium is a mythological name, coming from a Greek word meaning the moon.) It is a quite rare substance, very much resembling sulphur. It has several allotropic forms. It burns, giving off fumes smelling like rotten cabbage. It forms several oxides and acids, which are not of enough importance (on account of rarity) to be mentioned here.

Tellurium (atomic weight, 129; symbol, Te).—A still rarer substance than the preceding. It was discovered in 1782 by Müller, and named by him (from a Latin word meaning the earth). It is frequently classed with the metals, but its striking resemblance to sulphur and selenium seems to indicate that it should come under the head of non-metals. It forms several oxides, acids, and other salts, which are not of importance enough to be mentioned here.

Sulphur, selenium, and tellurium resemble each other in very many particulars—in regard to their action when heated, when burning, in combination with other elements, etc. The atomic weight of sulphur is 32, of tellurium 129, and of selenium 79·5, not very far from a mean between the other two. The specific gravity of the first is 2, of the third 6·25, and of the second 4·5, nearly a mean. Indeed, all the characteristics seem to unite to make a very clearly marked class of these three substances.

Phosphorus (atomic weight, 31; symbol, P).—A quite important substance, never found in nature in the free state. It was discovered by Brand, of Hamburg, in 1669. It is a soft, wax-like, semi-transparent, whitish substance, becoming hard and brittle at low temperatures. It is very inflammable, taking fire in the air very easily (especially if it contains impurities). When exposed to the air it slowly combines with oxygen, giving off white fumes, which, in a darkened room, show phosphorescent, pale greenish light; hence its name (phosphorus being derived from two Greek words, meaning light producing). It melts at 111° Fah., forming a transparent liquid, boiling at 554°. It is not soluble in alcohol, ether, or water, but slightly so in oils, and quite soluble in carbon disulphide. On account of its inflammability when exposed to the air, the least amount of friction often being sufficient to ignite it, it should always be kept and cut under water. A remarkable characteristic of phosphorus is the variety of forms it may exist in. The transparent variety has already been mentioned; when exposed to light under water it becomes white and less fusible. When melted phosphorus is suddenly cooled, a black and opaque variety is produced; or by heating near to its boiling point, and suddenly cooling, a viscous variety is produced; and if exposed to a temperature of about 460° in a gas which cannot affect the phosphorus chemically, a fifth modification, called red, or amorphous, phosphorus, is formed. This form differs very much from the transparent variety. It is insoluble in carbon disulphide. It emits no odor when exposed to the air, and does not take fire until heated to about 500°. Phosphorus acts as a very powerful irritant poison when taken internally.

But two oxides of phosphorus will be spoken of; phosphoric trioxide, P_2O_3, and phosphoric pentoxide, P_2O_5.

Phosphoric trioxide, or *phosphorus anhydride*, P_2O_3, is formed when phosphorus is burnt in a limited supply of dry air; it unites with the greatest ease with water to form hydric phosphate, or phosphorous acid, H_3PO_3 ($P_2O_3 + 3H_2O = 2H_3PO_3$), which acid forms a series of salts called phosphites.

Phosphoric pentoxide, or *phosphoric anhydride* (P_2O_5), is formed when phosphorus is burnt in an excess of air. This anhydride forms three acids when combined with water.

Hydric metaphosphate, *monohydric phosphate*, or *metaphosphoric acid* (HPO_3), formed thus: $P_2O_5 + H_2O = 2HPO_3$, which forms with bases salts called metaphosphates.

Tetrahydric pyrophosphate, or *pyrophosphoric acid* ($H_4P_2O_7$), formed thus: $P_2O_5 + 2H_2O = H_4P_2O_7$, which forms salts called pyrophosphates.

Trihydric phosphate, or *triphosphoric acid*, or *tribasic phosphoric acid* (H_3PO_4), formed thus: $P_2O_5 + 3H_2O = 2H_3PO_4$, which forms salts called phosphates. The acid being tribasic, it can form three kinds of salts, basic, neutral, or acid. One example will serve to explain; take the phosphates of sodium. It is plain that we can have three of these phosphates, according as the sodium replaces one, two, or all of the atoms of the hydrogen of the acid, thus: H_2NaPO_4, HNa_2PO_4, and Na_3PO_4. These would be Na_3PO_4, trisodium phosphate, basic; HNa_2PO_4 hydric disodium phosphate, neutral; H_2NaPO_4, dihydric sodium phosphate, acid. Salts formed like the last mentioned are often called biphosphates or superphosphates. Besides these there is a hypophosphorous acid, H_3PO_2, forming hypophosphites, whose anhydride has never been obtained in a free state. There are also three compounds of phosphorus and hydrogen: H_3P, phosphureted hydrogen gas, a gas; H_3P, liquid phosphide of hydrogen, a liquid; and HP_2, solid phosphide of hydrogen, a solid. There are also several other compounds of phosphorus—for instance, with chlorine, bromine, iodine, etc.—which are not of sufficient importance to be mentioned here.

Silicon or *Silicium* (atomic weight, 28; symbol, Si).—An element which, next to oxygen, is the most widely distributed of any on the face of the globe, and when combined with oxygen, as it always occurs, it never being found in the free state in nature, forms a substance which is the most abundant component of the earth's crust. It is the essential constituent of quartz, flint, silex (hence its name). Silicon may be obtained in the free state in any of three forms, the amorphous, the graphitoid, or the crystalline. Under the first form it exists as a brown amorphous powder, insoluble in water; when heated in the air, or oxygen, it burns brilliantly, forming silica. The graphitoid form may be obtained from this by heating, when it becomes very much smaller, and of a much greater specific gravity; will not burn even when intensely heated. The crystallized variety is obtained by fusing the silicon under certain conditions, and allowing it to cool slowly; it is then hard enough to cut glass.

There is but one oxide of silicon known: *Silicic dioxide*, *silicic anhydride*, or *silica* (SiO_2), the substance that is frequently called silicic acid, and which in the mineral kingdom can almost be said to be omnipresent. In its purest forms we have it glittering in the rock crystal quartz; color this purple with manganese, and we have the amethyst; color it red or brown with oxide of iron, and we have the carnelian; color it yellow, and we have the false topaz. In all of these the silica is the chief constituent; it is also found in nature in an amorphous form, that is, uncrystallized. Of the latter class the agate is typical. There are also the jasper, silex, or flint, etc.

Crystallized silica is attacked by no acid with the exception of hydrofluoric; it is very refractory before the blowpipe, requiring the most intense heat of the oxyhydrogen flame to melt it, when it fuses to a transparent globule. If it be mixed, however, with sodium carbonate, and then heated, it fuses with comparative ease, forming a silicate of sodium, "soluble glass," which, if boiled with water, will dissolve. Upon the addition of hydrochloric acid, the *true* silicic acid (H_4SiO_4) will partly separate as a gelatinous mass, and partly remain in solution. If, however, this solution be evaporated to dryness, and heated for some time at a temperature just a little above the boiling point of water (that is to say, if it be "brought to hard dryness"), and then some hydrochloric acid be added, the silica will remain undissolved as a white amorphous powder, which cannot be again obtained in solution without the foregoing operation be repeated. The silicic acid before mentioned, H_4SiO_4 ($SiO_2 + 2H_2O$), forms with bases silicates. But the subject of silicates will not be entered upon here; suffice it to say that there are but few subjects in chemistry more complicated. The acid represented by the formula H_4SiO_4 is not the only acid that the silicic anhydride forms; far from it. And as for silicates, there hardly seems to be an end of them; they are called sub-, mono-, bi-, poly-silicates; ortho-, meta-, acid silicates, and so on. It would be useless, as well as out of place, to describe them here. They are fortunately not encountered in one's everyday walks in chemistry, without one should chance to walk in that direction. There are other compounds of silicon, but they are not worth noticing.

Boron (atomic weight, 11; symbol B).—We will close the series of elements which we have included under the head "non-metallic" with boron. It is never found in nature in a free state, but combined with oxygen, or with oxygen and sodium, the latter being called borax, from which word the name boron is derived. In the same manner as carbon and silicon, it exists in three forms, amorphous, graphitoidal, and crystallized. When in the amorphous form, it is of a dull, olive-green color, soils the fingers, dissolves slightly in water, forming a greenish solution, from which it is precipitated unchanged by ammonium chloride. Boron, when crystallized, has a higher specific gravity than when amorphous. The crystals, when pure, are colorless, but generally, however, from impurities, are of a yellowish cast. They are very hard, sufficiently so to scratch the ruby, and even to wear away the diamond. Boron, when strongly heated in oxygen or chlorine, burns with beautiful scintillations, forming either the oxide or chloride, as the case may be.

Boracic anhydride (B_2O_3), commonly called boracic acid, is the only compound of boron and oxygen known. It is found in great quantities in California, where it is utilized. It is also obtained in Tuscany. When in regions of volcanic action it issues from the ground in a free state, mixed with sulphureted hydrogen, the gas is conducted into large basins containing water; the water absorbing the gas, the steam accompanying it is made use of for the evaporating of the water; crude acid crystallizes from the concentrated solution. This crude acid is purified by treating it with sodium carbonate until effervescence ceases, borax being formed (borax is an acid-borate of sodium). This borax is obtained nearly pure by crystallization. It is again dissolved, and enough sulphuric acid added to form sodium sulphate of the sodium present. The boracic acid being left in a free state, is then obtained by crystallization, in a state nearly pure, containing, however, some sulphuric acid, from which it is freed by fusion. The crystals thus obtained are boracic acid (HBO_2, H_2O), being composed of boracic anhydride and water ($B_2O_3 + H_2O = 2HBO_2$). When heated it loses water, being converted into the anhydride, finally fusing to a transparent glass. Boracic acid is but slightly soluble in cold water, but freely so in hot. It forms with bases salts called borates, and has apparently the property of imparting to them ready fusibility, so that many of the borates make valuable fluxes, and are utilized accordingly.

Borax, *acid borate of sodium*, or *biborate of sodium* ($Na_2B_4O_7, 10H_2O$), is probably the most important, certainly the commonest, of all the borates. It is obtained in large crystals, which, as will be seen by the formula given above, contain as much as ten molecules of water of crystallization to the molecule of borax; these are, of course, given up when the substance is heated. It swells up to a remarkable extent in the process, finally contracting to a smaller bulk than it first had, and forming a glassy transparent substance, which is anhydrous (containing no water), and is known as "borax glass." The power of dissolving metallic oxides, possessed by borax to a remarkable degree, renders it of importance in several connections. Most of these oxides have the power of imparting to the borax bead* a characteristic color. Hence borax is an important reagent in blowpipe analysis. Some of the borax being fused in the loop of a light platinum wire, and a small quantity of the substance to be tested fused into this bead by the resulting color, very often the composition of the substance may be determined. Its power of dissolving oxides is utilized in the soldering of easily oxidizable metals. The borax is sprinkled on the metal, and the heat melts the borax as well as the solder. The melted borax then dissolves any of the oxidized metal that may be formed, rendering the surfaces clean, otherwise the oxide would interfere with the perfect joining of the two parts. Borax, like other borates (and like boracic acid also), is used often as a flux; in the enameling and coloring of porcelain, for example.

BRITISH BEE EXHIBITION.

THE fourth show of the British Bee-Keepers' Association was lately held at the Royal Horticultural Gardens, South Kensington. Sir John Lubbock was the first President of this society, and he has been succeeded by Lady Burdett-Coutts. Its objects are the advancement of bee-keeping as a means of bettering the condition of cottagers, and the advocacy of humanity to the honey bee. The secret of modern apiculture is to avoid the ancient error of killing the bees before their store of honey was rifled. Now they are by various ingenious processes deprived of the honey which they produce in quantities much greater than their own needs demand; they are fed in bad seasons; kept warm in winter; and in the summer taken on pleasant excursions among the flowers.

Nothing is more remarkable in this branch of the husbandry concerned with bees than the voyage undertaken by Capt. Perrine, a well-known Chicago bee master, who was in this country last year to buy Ligurian queens. He puts his bee hives on a barge and tugs them up the Mississippi, gradually advancing according to the season in which different flowers are in bloom. In Scotland hives may be met traveling in carts to the heather. On the Nile the bee boats voyage by night, stopping for the day at places where there is good pasturage. The Italians carry their bees up the stream of the Po, and when the boats have sunk to a certain depth it is taken as a sign that the hives are sufficiently full, and the barges laden with their honeycombs float down again to the home pastures. The bees richly repay such attentions as these, not only in their tribute of honey and wax, but by their service to the flowers they milk of honey.

In the process of removing the precious deposit from the blossoms, they carry pollen from flower to flower, so that the fruit trees of an orchard bear best where bees are busy among them when the blossoms are out. The New Zealand Government are to import this winter, through Mr. Neighbour, a quantity of bumble bees to enable the colony to grow its own red clover. This valuable food for stock is dependent upon the visits of the bumble bee for reproducing itself, and the seed has to be imported year by year, by colonies which have no bees. It is the field bees which are useful in this respect; the several varieties of hive bees are valuable in other ways.

Among the most esteemed sorts of hive-dwellers are the recently introduced Cyprian bees. The *British Bee Journal* says that Herr Corri speaks most highly of the good qualities of the bees of Cyprus, and considers them in advance of any other bees he has cultivated. In this opinion he is borne out by Count Rudolph Kollowrat of Tabor. Specimens of the Cyprian bees are exhibited by Mr. Neighbour at the great show. The German pastor Dathe writes of them: "The Cyprians are diligent, and are, like the Carniolan, a fancy breed. The bees which the greater number of masters rear in this country are the black, and the yellow-banded Ligurian or Italian bee."

The most important part of the bee show is the exhibition of those methods of "driving" and manipulation by which the bees are induced to leave their hives. A competition for prizes in driving was held in a tent which had an inner inclosure of netting, so that the process could be observed without fear of being stung. The bees are first

* The borax "bead" is a small globule of borax, made by melting some borax in a small loop of light platinum wire (the loop forming a support).

induced to fill themselves with honey, and are then drummed out of the hive into a skep, from which they are shaken into the new hive.

Many improved bar and frame hives were exhibited which make the skill of the driver unnecessary, since the bees can be easily shaken out of them. In these a thin plate of wax is inserted to guide the bees in making their cells. They take advantage of the wall thus provided and build against it. Their time is saved and the combs are regularly built. A machine to produce these guiding walls by rapidly passing wax under the roller is exhibited this year. "Supers" are also shown. These are placed above the hives, and are removed as soon as they are filled with honey. Mr. John Hunter, the well-known apiarian, shows little American supers, just large enough to hold a pound of honeycomb. "Slingers," or "extractors," in which the comb is placed and turned rapidly round till the honey is expelled by centrifugal force, are also shown.

Splendid honey comes from Dumfries and Herts. It is shown in all colors—light, if the bees have fed on white clover in Ayrshire, darker if their pasturage has been upon the lime blossoms of Highgate, and so on.

Among curiosities is a Portuguese hive of bark, exhibited by the Rev. F. T. Scott, and a quantity of honey produced on an upper floor in the Strand by bees belonging to Mr. Thurston, which foraged on the flowers of the embankment, or flew across the Thames to the learned shades of Lambeth. An interesting collection is that of the flowers from which bees chiefly gather their food—the spiked teasle, the meadow-sweet, the thyme which gave its flavor to the honey of Hymettus, the white nettle, the fragrant mignonnette and lavender, with borage, good also in claret cup.

The exhibition was enlivened on the first day by a discussion, opened by the Rev. J. D. Glennie, on questions interesting to bee masters. One of these was, "How far is the process which leads to swarming initiated by and carried out with the good will of the old queen?" The prevalent opinion was that the queen did not leave the hive willingly; one apiarian, indeed, had seen her forcibly led out between two resolute advisers. The President of the society, Lady Burdett-Coutts, was present during the day, but did not distribute the prizes as announced.

The honorary Secretary, Mr. Peel, announced in his address preceding the distribution of prizes that the show had been successful and well attended, and that it was expected to be repeated next year in the same place. Mr. John Hunter, member of the Committee, delivered a lecture to the visitors in which he explained the improved methods of bee-keeping by the use of bar and frame hives, and the process of driving the bees and transferring them from one hive to another while the honey was "slung" out of the combs of the first. The combs can, after this operation, be restored to be filled again by the industrious insects, who under older methods of culture would have been stifled with sulphur, while their successors would have had to waste precious days of summer in building up fresh honey combs from the beginning.

Several experienced bee masters gave examples of manipulation, guiding the bees like a flock of sheep into new hives, and rapidly selecting the queen, whom the rest always followed, from the drones and workers. Examples of combining the inmates of several hives into one were shown, and it was stated that strange bees would always be received into a new hive if they brought honey with them. If, however, they could not thus pay their footing, they would be driven away like the drones (the male bees), which, after being fed for a brief season with the richest sirups by the workers, are expelled to perish of hunger and cold at the mouth of the hive.

The following were the principal prizes distributed at the close of the day: For the best hive for observation purposes, all combs visible on both sides, exhibited stocked with bees and their queen. For the best movable comb hive, including covering and stand. For the most economical (best and cheapest) complete hive, on the movable comb principle, for cottagers' use, including cover and floor board. For a hive for general use, on an entirely new and approved principle. For the best and cheapest supers for general use in an apiary. For the cheapest, neatest, and best supers for producing honey comb in a salable form. For the best stock of Ligurian or other foreign bees. For the best stock of pure English bees. For the largest and best harvest of honey in the comb and from one stock of bees, under any system or combination of systems. For the best super of honey, the super to be of wood, straw, or of wood in combination with glass or straw. For the best glass super of honey.

For the best exhibition of honey in supers, or sections of supers, separable, and each not more than three pounds in weight, the total weight not less than twelve pounds. For the best section in the comb, weighing not more than three pounds. For the best exhibition of run or extracted honey, in glasses of five pounds to ten pounds each. For the largest and best exhibition of super honey in comb, the property of one cottager, and gathered by his own bees. For the best super of honey shown by a cottager. For the best exhibition of run honey in glass jars, containing five pounds to ten pounds each, shown by a cottager. For the best mead or beer made from honey, with recipe attached. For the best and largest collection of hives, bee furniture, bee gear, and bee-keeper's necessaries, no two articles alike. For the best honey extractor. For the finest sample of pure beeswax, not less than three pounds in weight. For any new invention calculated in the opinion of the judges to advance the culture of bees. For the best microscopic slides illustrating the natural history of the honey bee. For the best and largest display of British bee flora. For the best and cheapest honey jars, with covers and fastenings complete, to contain one pound and two pounds each of extracted honey. For the competitor who in the neatest, quickest, and most complete manner drove out the bees from a straw skep and captured the queen. In addition to the principal distinctions, second, third, fourth, fifth, sixth, and even seventh prizes, as well as high commendations, were awarded to exhibitors. Honey from Holloy Lodge, the residence of Lady Burdett-Coutts, was among the highly commended.

COMB-FOUNDATION.

By S. D. Riegel.

I have made extensive use of comb-foundation this season, and have sold a considerable amount to my neighbors, and from my own as well as my neighbors' experiments I am surprised that so little has been said or published in its favor by those who gave it a fair test last year. Whether it was from a motive of selfishness or from any improper use of it, thereby making it only partially a success, I am not able to say. I hope it has not been from the first cause. I am so fully convinced of its value in the brood frames (if properly manufactured and used) that I think no bee-keeper can do without it, for he can certainly not compete with those who use it. It is certainly the greatest improvement which has been made in bee-culture since the introduction of the honey extractor.

It is no less surprising to me that those bee-keepers who claim to have given it a fair trial last year have left us so much in the dark as to its proper use or manipulation in the hive. When I first thought of using foundation I sent to one of the leading apiarians of the country for a sample put in a frame. It was full width of the frame and about nine inches down in length. I followed the pattern with about a half dozen frames; put them into several colonies of bees, one or two frames in each. After waiting a day or more I opened the hives, and to my surprise and disgust I found nearly all my foundation knocked into beeswax and piled on a heap on the bottom board of the hives. So much for scientific bee-culture. But, as the old adage runs, "If you don't at first succeed, etc." I concluded I would try a plan of my own.

I buy the foundation cut in sheets 12x12 inches, and I cut each sheet in four pieces, by laying a ruler from one upper corner to the opposite lower corner, and without moving the sheet lay the ruler to the upper corners and cut again. This gives me four pieces, each being twelve inches across the side which is attached to the frame, and running to a point in the center, six inches in length. This leaves the shape of the foundation in the frames more nearly like natural built combs than any other shape that I have heard of being recommended, or that I have tried. The bees in lengthening down the foundation will keep it very nearly the same shape that it was cut in the first place, keeping the point ahead until it touches the bottom bar, and always finishing by fastening the combs down each side last. This prevents their building the combs zigzag across the lower ends, as they do when the whole width of comb is built down about even. The straightest combs that I have ever seen have been built from foundation arranged in this way. I usually keep a lot of frames filled with the foundation, and whenever I have a place that I can use a frame I put in one of these. In making artificial swarms I draw a frame from a strong stock and put in the new hive, then fill in frames with foundation in. I use nine frames in my hives; that gives me eight frames filled with the foundation, besides the one drawn from the old stock. This gives me about 15,500 cells, which a strong colony in the midst of the honey harvest will draw out in from two to four days ready for the queen to deposit eggs in or for storing honey, and all the combs will be straight.

Now it is plain to see that foundation used as above recommended will pay, and largely pay, the bee-keeper who will use it properly. The cost of the eight pieces of foundation will be about twenty cents.

Now a few words about comb-foundation in section frames of boxes. The advantages gained by the use of comb-foundation for starters for surplus in the comb are no less than for brood combs, if we can only overcome the objectionable points which have been made by some of our bee-keepers, viz.: that the bees do not always thin out the foundation, and that the wax used is often extracted from combs which contain substances such as bee-bread, moth worms, and not unfrequently brood in different stages of development. Now these are objections which we dare not overlook if we expect to supply our markets with honey in the comb that is unobjectionable as it has been in the past.

I will give you my views as to how this difficulty can be obviated. Every extensive bee-keeper should have a comb-foundation machine and manufacture his own foundation, then he could assort all his combs intended for wax, and use only the whitest and cleanest combs for extracting into wax for starters for surplus in the comb, and this should be bleached and then made into thin foundation. When made in this way no objections can or will be made to its use. This is the plan I expect to adopt next season if I make comb honey extensively.—*Bee-Keepers' Magazine.*

PARIS GREEN—ARSENITE OF COPPER.

A Canadian correspondent of the *Agricultural Gazette* says: Some one asks, "What is Paris green?" having reference to its use in destroying the Colorado beetle, as a remedy for that pest, to be applied by children of ten and twelve years old. Paris green is arsenic in soluble compound with copper. In our mining researches and experimenting on the gold-producing pyrites of the township of Marmora, Ontario, we have manufactured "lots" of Paris green from this mineral, which is usually called "Mispickel." I mention this merely to show my intimate acquaintance with several preparations of arsenic. I always worked among them with my own hands, and was engaged at it for some years at intervals, and never ordered men to do what I was afraid to do myself.

Paris green is perhaps one of the most deadly poisons—worse even than the white arsenic of the shops—as its peculiar state, when combined with copper, renders it much more readily assimilated by the stomach. Neither I nor my men were ever injured by it in the least; but we were, of course, very careful, and well understood its properties. If great care be exercised not to inhale the dry, green powder, or drink any of the soluble arsenic before being combined with the copper or afterward, people may be readily employed in handling it without danger to life. Still it is altogether too virulent a poison for ignorant people or children to meddle with or apply it to any use.

As to any substitutes for the destruction of the Colorado beetle, none as yet have been found at all effective. Numbers of nostrums have been tried, but without any certain effect being produced on the insect, and at the same time harmless to the potato.

It is not the object of the present article to go into the manufacture of Paris green—this is perfectly well known to chemists and manufacturers, and as it cannot be "home made" with safety, such a description will not be likely to interest the farmer—my object being simply to impart my experience to those who have not yet had the practice in its use that we in Canada most unfortunately have in contending successfully with the Colorado beetle.

Numbers of plans have been tried and published for applying the poison to potatoes. It has been mixed with twelve to twenty times its weight in flour, and so dusted over the plant. This is a very defective and at the same time very expensive plan. First, it is certain to fly about and attach itself to the hands and clothes of the operator—unless the atmosphere is perfectly calm—and if the wind blows at all excessively, a light cloud of it will be wafted to the mouth and nostrils with certain injurious effects to the man using it. Second, it does not adhere to the leaf of the plant as effectively as other modes of applying it do. Third, it is apt to blow away as soon as the wind rises, even if it is applied when the atmosphere is perfectly calm; and fourth, the flour costs much money, and the poison is not so thinly and evenly distributed as by other means.

Other dry menstruums have been used, as being less expensive, but they are all open to the same objections. To meet these objections, water has been used after the powder has been thoroughly mixed with it. This is a much better plan, and far more effective, but is still open to grave objections. The powder is almost as heavy as white lead, and consequently cannot be long kept in suspension in water during its application and while being sprinkled over the vines, and when the sun dries the leaves or a shower of rain falls there are many parts on which the insect can feed with impunity, especially if it be a windy season.

These objections are all met by the following: Take two hundred gallons of water quite free from the smallest motes or particles of roots—unless quite free it must be carefully strained; add to this sufficient thin (perfectly free from lumps) flour paste, enough to make the water slightly gelatinous; strain carefully before mixing in the large vessel—this is most important; take two and a half pounds of pure, unadulterated Paris green and mix with some water; strain this also into the larger vessel, and with a paddle thoroughly stir up all together. If the water be made of the required consistency it will support by suspension a very large portion, if not all, of the green powder—at all events it will do so by agitating it now and then, and if the powder be perfectly free in its particles and from heavy adulteration there will not be found much difficulty in applying the liquor on account of heavy deposits.

Now commence to use the mixture. Fill an ordinary 2-gallon watering pot (having first ascertained that the holes in the rose are fine enough to let a very fine stream pass through them), which should also be so constructed as to avoid sprinkling a wider space than the row of potato crops. A little practice will enable a sensible man to walk rapidly along the row and apply the two gallons to about 200 feet of each row, and the 200 gallons will be found sufficient for an acre, and one dressing. Of course no further rule can be laid down, as much depends on the size of the holes in the rose and the speed of the operator.

The thin paste is cheap, and supports the powder in suspension during its application. It also affords an adhesive medium to cause the powder to adhere, in a very thin particle, to the leaves. The stalks are not usually eaten by the insect. It also prevents the loss of the powder by a shower of rain or a windy day. And last, but not least, it retains the poison on the leaf of the potato until the younger insect feeds on it, which would not be the case if some adhesive matter were not used.

It will readily be understood that these insects are at the same time in various states and sizes and ages on the same plant. To the old ones outward applications are not always effectual directly, but they must eat somewhat, sooner or later; but to the younger insects in their various stages it is death, both where applied outwardly and taken into the stomach. More particularly, however, is it applicable in the latter case, as all the larvæ are most ravenous, and are continually eating.

So deadly is this application, that a row of moderately-sized potato tops on which thousands of insects are feeding to-day will be altogether cleared and relieved of them in twenty-four hours afterward. Some few of the old ones will fall down and crawl up again, but their fate is only a question of time; they must eat, and do not appear to be able to discriminate between the leaf that is poisoned and the one that is not.

I have often seen one single application most effectually cure the evil for that time, and result in a splendid crop of Early Rose potatoes. Of course, if the late varieties are sown, the beetle may have a chance of a second attack from outside sources or from unhatched eggs or eggs laid later by some old female who refuses to feed when she ought to have done so, but a second application is all that is ever required.

Cattle must not on any consideration be admitted into the field where Paris green has been used, not even during the same year, or so long as any vegetation exists and the land remains unplowed. Afterward there is not the slightest danger to "man or beast."

The leaves of the potatoes are the only part affected to the fullest extent. The stalks are, of course, somewhat protected, and, not usually being eaten by the insect, need not be so carefully watered.

The greens do not absorb in any perceptible degree any of the poison, and I believe, if thoroughly washed and entirely cleansed from all external application, could be eaten by cattle with impunity. Of course, I would not advise such a course, as the rough nature of the potato top and stem would render the perfect removal of all poison very difficult.

The tubers are absolutely free from any taint of arsenic, and no instance of poisoning has ever occurred from their use. The very best American analytical chemists have failed in detecting any arsenic in the roots, notwithstanding it has been tried many times. I can imagine that if potatoes are dug soon after the application of the poison, and are produced in a clayey, sticky soil, and consequently may have a minute portion of the poison adhering to them and the soft, sticky clay from which they are dug, under such a combination of circumstances an analysis of a quantity of unwashed tubers would show more or less proportion of arsenic. But all these contingencies do not often arise, and potatoes are, or ought to be, thoroughly washed in a revolving cage before being sold, and in a climate like England ought to be prohibited being offered for sale before being subjected to this process. If this was done the last vestige of chance of evil arising from the use of Paris green would be done away with.

Nor does the most accurate analysis show any accumulative effect in the soil or in any of the crops raised following the application of the poison, the quantity of arsenic is so small, and the quantity of earth among which it is mixed is so large after plowing, added to which vegetation does not seem to possess the power of absorbing arsenic to any deleterious point. In my works I have seen the white powder lying on the grass like fine snow, and I never could detect any injury to the plants or to animals feeding over the same ground a few months afterward. But I have known many cases of poisoning of cattle to occur in potato fields, and therefore earnestly advise caution even beyond danger point.

Belgian Burnishing Powder.—A burnishing powder in use in Belgium is composed of ½ lb. fine chalk, 2 ozs. pipe clay, 2 ozs. white lead, ¾ oz. magnesia (carbonate), and the same quantity of jeweler's rouge.